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ADAPTATION–LEVEL AS A BASIS FOR A QUANTITATIVE THEORY OF FRAMES OF REFERENCE

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INTRODUCTION

The fact that all judgments are made with respect to a frame of reference was first postulated and experimentally demonstrated in psychophysical experiments and later extended to attitude-formation and social behavior generally. The terms standard, norm, value, anchor, and frame of reference are now as widely employed as conditioning, Gestalt, trial and error, past experience and association. Indeed it is evident that terms embodying the notion of frames of reference are being used descriptively and as explanatory principles in place of many of the time-honored concepts just mentioned. Thus Sherif and Cantril (23, p. 295), who agree with Allport (1, p. 798) that 'attitude' is probably the most central and indispensable concept in all psychology, find its ultimate significance in frames of reference when they say: ". . . the socialization which occurs when an individual becomes a member of a group consists mainly in the achievement of conformity in experience and behavior to social values, standards, or norms already established" (23, p. 296). But when we inquire more closely' into what these terms precisely refer ·to and how they can be made amenable to experimental control, we find no concrete definitions

even in psychophysical studies where their meaning should be most clear. As Rogers has pointed out: "Few concepts are so widely used, with so few attempts at definition or explanation, as this concept of frame of reference" (21, p. 5). It is safe to say that the more complex the behavior to which these newer terms are applied, the vaguer do they appear to be.

A more concrete approach to frames of reference would clarify a number of basic problems not only in psychophysics and perception but would also contribute materially to other fields in so far as there is a marked tendency for workers in social, abnormal and personality psychology to take over concepts from perception and psychophysics. As Sherif puts it: "The first important step in social psychology is to analyze *the way we react to a perceptual field*. Further elaborations of the psychology of thinking, feeling and acting follow when the properties of perception have been described" (22, p. 78). Yet Sherif's own discussion of norms in his work dealing with the autokinetic phenomenon, which is basic both to his and Cantril's approach to social psychology, is by no means satisfactory. Discussing autokinesis, Sherif states that ". . . when individuals perceive movements which

lack any other standard of comparison, *they subjectively establish a range of extent and a point (a standard or norm) within that range which is peculiar to the individual.* . . . This subjectively established norm serves as a reference point with which each successive experienced movement is compared and judged to be short, long, medium—within the range peculiar to the subject" (22, p. 96). But a norm which is 'subjective' and 'peculiar to the subject' can hardly be said to satisfy the requirements of a scientific definition on which social psychology can firmly rest. Nor can such an account of norms and standards serve as a basis for work in studies of perception. A concrete, quantitative theory of frames of reference is now definitely in order to tie together a large number of experimental observations and to provide a theoretical framework within which problems may be reformulated and new research initiated.

In this article an attempt will be made to show how the theory of adaptation-level can provide a quantitative approach to phenomena of level, frames of reference, norms, and kindred concepts. Adaptation-level, defined operationally in terms of the stimulus evoking a neutral or indifferent response, can be quantitatively determined, and since the structure of the behavioral field is fixed by the position of the neutral point, the frame of reference is completely defined once its value is known. The quantitative theory takes account of all shades of judgment expressed either in qualitative or numerical categories and is applicable not only to psychophysical data but also more generally to any type of response involving objectively specifiable stimuli. In view of the fact that many experiments in social, abnormal, and other fields of psychology are performed with quantifiable stimuli, the theory finds application quite generally. Before turning to the mathematical formulation, let us first consider the experimental origins of the theory.

GENERALIZATIONS FROM EXPERIMENTATION

The concept of adaptation-level originated in studies of constancy, contrast, adaptation, and color conversion (5, 6). Prior to this theory it was commonly held that constancy was different from contrast and adaptation, the three being more or less independent. Occasionally paradoxical relations were thought to exist among them, for example, at one time constancy might aid contrast, at another hinder it (12). Failure to give credit to the extremely labile possibilities of the visual mechanism and to investigate systematically fundamental factors such as role of background delayed formulation of a theory linking constancy with other, well-established visual phenomena. When it was discovered that changes in reflectance of background can completely obliterate or reverse constancy effects and that visual tolerance to changes in quality of illumination depends largely on shifts in position of the neutral point of the eye the way was opened for a more dynamic view of visual functioning. The fact that background effects can be observed in extremely brief exposures (13, 8, 6) indicates that retinal interaction occurs in times so short as to be negligible in many cases. Taken together these, and other facts, indicate that we must regard the visual mechanism as a system capable of extremely rapid change, any momentary state of the system representing a quasi-stationary process in dynamic equilibrium. The key to an understanding of classical adaptation, contrast effects, and constancy, under the restricted range of conditions first investigated by Katz (13), as well as the phenomena of color conversion (5, 6), was found to lie in shifts of the achromatic point of the

TABLE 1

HUE, LIGHTNESS, AND SATURATION OF 19 DAYLIGHT GRAY SAMPLES HAVING REFLECTANCES FROM 3 TO 80 PER CENT VIEWED IN FOUR STRONGLY CHROMATIC ILLUMINANTS ON DAYLIGHT WHITE, GRAY, AND BLACK BACKGROUNDS

(Hues are given as Red, Green, Yellow, or Blue with small letters indicating the minor component in binary hues. Lightness (numerator) and saturation (denominator) are in terms of a 0–10 scale)

R	Red Illuminant			Green Illuminant		
	W Bkgd.	G Bkgd.	B Bkgd.	W Bkgd.	G Bkgd.	B Bkgd.
0.80	R 9.0/2.0	YR8.0/8.0	R 8.0/8.5	yG 9.0/2.0	yG 8.0/7.0	yG9.0/7.0
.52	R 7.0/3.0	YR7.5/6.0	R 8.0/8.5	yG 8.0/1.0	yG 7.5/6.0	yG8.0/8.0
.39	R 7.0/1.0	YR7.0/4.0	R 7.0/8.0	A 8.0/0.0	yG 7.0/4.0	yG8.0/8.0
.34	R 7.0/1.0	YR7.0/4.0	R 7.0/8.0	A 7.5/0.0	yG 6.5/4.0	yG7.5/9.0
.27	A 5.0/0.0	yR 6.5/4.0	R 6.0/7.0	RB6.0/2.0	yG 6.0/1.0	yG7.0/7.0
.23	rB 5.0/1.0	yR 6.5/4.0	R 6.0/8.0	RB6.0/2.0	yG 6.0/0.5	yG7.0/7.0
.22	rB 5.0/1.0	yR 6.0/3.0	R 6.0/8.0	RB6.0/2.0	A 5.0/0.0	yG6.5/6.0
.17	rB 4.0/1.0	yR 5.5/3.0	R 6.0/7.0	RB5.0/3.0	A 5.0/0.0	yG7.0/5.0
.16	rB 4.0/1.0	yR 5.5/3.0	R 5.0/7.0	RB5.0/3.0	A 5.0/0.0	yG7.0/5.0
.15	B 5.0/2.0	yR 5.5/2.0	R 5.0/6.0	RB4.0/3.0	BR5.0/0.5	yG7.0/5.0
.13	B 4.0/2.0	yR 4.5/2.0	R 5.0/6.0	RB4.0/3.0	BR4.5/1.0	yG7.0/5.0
.13	B 3.0/2.0	yR 4.5/1.0	R 5.0/6.0	RB4.0/4.0	BR4.5/1.0	yG7.0/5.0
.11	B 3.0/2.0	yR 4.0/1.0	R 5.0/6.0	RB3.0/4.0	BR4.0/2.5	yG6.0/5.0
.10	BG3.0/4.0	A 4.0/0.0	R 4.0/4.0	RB3.0/5.0	BR3.0/5.0	yG4.0/3.0
.07	BG2.0/4.0	BG3.0/2.0	R 3.0/4.0	RB2.5/6.0	BR2.5/5.0	yG3.0/2.0
.07	BG2.0/4.0	BG3.0/4.0	R 3.0/4.0	RB2.5/6.0	BR2.5/6.0	yG3.0/2.0
.05	BG2.0/4.0	BG2.5/4.0	yR 4.0/4.0	RB2.0/7.0	BR2.0/6.0	yG2.0/1.0
.03	BG1.0/4.0	BG2.0/6.0	yR 2.0/1.0	RB1.0/9.0	BR1.5/7.0	A 1.0/0.0
.03	BG0.5/4.5	BG1.0/8.0	A 0.0/0.0	RB0.5/9.5	BR1.0/7.0	A 0.5/0.0

R	Blue Illuminant			Yellow Illuminant		
	W Bkgd.	G Bkgd.	B Bkgd.	W Bkgd.	G Bkgd.	B Bkgd.
0.80	B 9.0/1.0	B 8.5/3.0	B 9.0/3.0	Y 9.0/2.0	Y 8.0/7.0	Y 9.0/8.0
.52	rB 8.0/1.0	B 8.0/2.5	B 8.0/4.0	Y 6.0/1.0	Y 7.5/7.0	Y 9.0/8.0
.39	A 7.0/0.0	RB8.0/2.0	B 7.0/4.0	A 6.0/0.0	Y 7.0/4.0	Y 8.0/6.0
.34	R 6.0/1.0	RB7.0/2.0	B 7.0/4.0	RB8.0/1.0	Y 6.5/4.0	Y 8.0/6.0
.27	R 5.0/1.0	RB6.0/0.5	B 6.0/4.0	RB7.0/3.0	A 6.5/0.0	Y 7.0/4.0
.23	yR 6.0/0.5	RB5.5/0.5	B 6.0/4.0	RB7.0/3.0	A 6.0/0.0	Y 7.0/4.0
.22	yR 5.0/1.0	A 5.0/0.0	B 6.0/4.0	RB6.0/4.0	A 5.0/0.0	Y 7.0/4.0
.17	rY 5.0/1.0	A 4.0/0.0	B 5.0/3.0	RB6.0/5.0	RB5.0/1.5	Y 6.0/4.0
.16	Y 2.0/1.0	A 4.0/0.0	rB 5.0/3.0	RB6.0/5.0	RB5.0/1.5	Y 5.5/4.0
.15	Y 2.0/2.0	A 4.0/0.0	rB 6.0/4.0	RB5.0/5.0	RB4.0/2.0	Y 5.5/3.5
.13	Y 2.0/2.0	Y 3.0/0.5	rB 6.0/5.0	RB5.0/4.0	RB4.0/2.5	Y 5.5/3.5
.13	Y 2.0/2.0	Y 3.0/0.5	rB 6.0/5.0	RB5.0/5.0	RB3.5/2.5	Y 5.5/3.5
.11	Y 2.0/2.0	Y 3.0/0.5	rB 5.0/4.0	RB5.0/5.0	RB3.5/3.0	Y 5.0/3.0
.10	RY3.0/2.0	RY2.5/1.0	RB4.0/3.0	RB4.0/5.0	RB3.0/5.0	Y 4.0/3.0
.07	RY2.0/3.0	RY2.0/2.0	A 5.0/0.0	RB3.0/8.0	RB3.0/5.0	Y 3.5/2.0
.07	RY2.0/3.0	RY2.0/3.0	A 4.0/0.0	RB3.0/8.0	RB2.5/5.0	Y 3.0/2.0
.05	RY1.0/3.0	RY1.5/3.0	A 3.0/0.0	RB2.0/8.0	RB2.5/6.0	Y 3.0/2.0
.03	RY1.0/3.0	RY1.0/5.0	rY 2.0/1.0	RB1.0/8.0	RB2.0/7.0	rY 2.0/2.0
.03	RY0.5/2.0	RY0.5/5.5	rY 0.5/1.0	RB0.5/8.5	RB1.0/7.0	A 0.0/0.0

visual system with change in background reflectance and spectral energy distribution of stimulus and illuminant. The neutral point fixes the center of gravity of the frame of reference.

The way in which shifts of the neutral point furnish the clue to organization of the behavioral field is clearly shown by the data in Table 1 for vision and in Table 2 for lifted weights. Although different sense modalities are involved, the results are similar in principle in the two cases and enable us to draw conclusions having general applications. In Table 1 are given reports made by observing 19 daylight gray papers having reflectances ranging from 3 to 80 per cent when viewed on daylight white (80 per cent), gray (26 per cent), or black (3 per cent) backgrounds in four strongly chromatic illuminants, which were either red, green, yellow or blue. It is apparent that the hues of the papers depend as much upon the background reflectance as upon the quality of the illuminant. Saturation depends upon distance of sample reflectance from the reflectance reported achromatic or adaptation-level reflectance.

It is evident from the data of Table 1 that the key to the structure of the visual field lies in the position of the achromatic or neutral point. Knowing the position of this point and the spectral reflectance of any sample, we can predict its hue, lightness, and saturation. The structure of the visual 'frame of reference' can be concretely specified as follows: samples above the neutral point take the hue of the illuminant; samples below, take the after-image complementary hue; and samples near the achromatic region are either gray or very weakly saturated. That background in vision is similar to standards or anchors in lifted weights appears from the neutral points in Table 1: the average reflectance of stimuli reported

achromatic on 'white' background is 36 per cent, on 'gray' background, 19 per cent, and on 'black' background, 4.4 per cent. While the equilibrium point follows background reflectance, it is important, for theoretical purposes, to note that the two are not identical. If they were, then only after-image hues would be seen on white background since all samples would lie below the neutral point, and only illuminant hues would be visible on black background. Such a state of affairs could only mean the organism, by adapting to extremes, does not discriminate as well as it can if, as is the case, its point of equilibrium is below the highest reflectance and above the lowest. This lag has other important consequences we shall discuss later.

Organization of the behavioral field for lifted weights is strikingly similar to that for vision as shown in Table 2.[1] In this table are given averages of five series of observations by four observers under three different conditions. Five weights, ranging from 200 to 400 grams, were judged by method of single stimuli (so-called absolute method); also following a 900 or 90 gram standard. The qualitative responses were translated into a numerical scale for purposes of computation as follows: very, very heavy, 90; very heavy, 80; heavy, 70; medium-heavy, 60; medium, 50; medium-light, 40; light, 30; very light, 20; and very, very light, 10. From Table 2 it is seen that stimuli above the neutral point are judged as more or less heavy while stimuli below are judged as light. A heavy standard is analogous to white background in that the judgments are shifted toward the 'light' side; a light standard forces the judgments upward toward the heavy side and the method of single judgment

is analogous to gray background in that the judgments are more evenly balanced. Again the position of the neutral (medium) point provides the clue to the structuration of the field: with 900 gram standard the indifference point is 337 grams, with 90 it is 186 grams, and with the method of single stimuli the neutral point lies within the series but it should be noted it is well below center.[3] Here, as in vision, stimuli below adaptation-level are given opposite responses from those above, while those near it are reported neutral. Since adaptation-level tends to follow but does not coincide with the standards they play a role similar to background in vision. From Table 2 it is seen that

TABLE 2

JUDGMENTS OF LIFTED WEIGHTS AS A FUNCTION OF THE POSITION OF ADAPTATION-LEVEL

	Series Stimuli					
	200	250	300	350	400	A_{cale}
900 Standard						
Experimental	23	35	44	52	59	
Theoretical	23	35	44	52	59	337
Single Stimuli						
Experimental	34	50	63	74	82	
Theoretical	33	50	63	74	82	248
90 Standard						
Experimental	52	61	67	73	78	
Theoretical	52	61	67	73	78	186

the predicted values, based on the assumption that stimuli are judged with respect to adaptation-level rather than with respect to so-called standards or

[2] The fact that the indifference point is below the mean of the series has been known for a long time and has been treated at length in a previous article by the writer (7).

anchors, are in almost perfect agreement with the experimental data. The mathematical theory will be given below.

One value in Table 2 deserves special notice, namely, 59 for the 400 gram stimulus with the 900 gram standard. If judgments of stimuli depend only on their relation to the standard then a judgment of medium-heavy for this stimulus is paradoxical since it is less than half as heavy as the standard. Both classical theory and simple summative views of 'contrast' would lead us to expect a judgment of light. However, if, as we believe, judgments are made with respect to the pooled effect of all stimuli, namely, with respect to adaptation-level, the value of which is 337 grams, the medium-heavy judgment for the 400 gram stimulus is not paradoxical in our theory.[3]

It is evident from the preceding discussion that in both vision and kinaesthesis the position of the neutral point [4]

[3] The position taken here is quite different from views which regard extreme stimuli as the reference points. Although B. R. Philip (17) holds that extreme stimuli become reference points because of their clarity other aspects of his theory are very close to our position. Our theory differs also from theories which regard the changes associated with shifts in adaptation-level as due to changes in scale value. Hunt (10) and Hunt and Volkmann (11) have described many interesting phenomena in terms of changes in judgmental-scale. This account does not seem adequate in that it does not do justice to the striking changes in sensory character of stimuli as the neutral point moves. We have to account for re-structuration of the whole behavioral field with changes in adaptation-level.

[4] The word 'point' used here and later should not be taken to mean that adaptation-level coincides with a single value of stimulus. Rather, adaptation-level is a *region* of neutrality or indifference as shown by the reports of achromaticity in Table 1. The width of this region is a function of many conditions. Tresselt and Volkmann (25) used the width of the neutral region to measure uniformity of opinion in a group.

determines how all stimuli are judged, and hence fixes the frame of reference. That this generalization applies much more generally seems more than likely in view of the fact that shifts in scale values have been found with affective stimuli, aesthetic objects, and verbal meanings when so-called anchors, norms, and standards are changed.[5] If, for convenience, we refer to stimuli above the neutral point as positive and to those below as negative, then we can say that when adaptation-level is high, positive responses are few or absent and negative ones predominate; conversely, when adaptation-level is low, negative responses are few or absent and positive ones predominate. When the adaptation-level has an intermediate value, as is the case when stimuli are judged singly or the standard is near the center of the series, positive, negative, and neutral responses are more or less balanced.[6] Position of the neutral point

[5] Beebe-Center (2) has investigated affective stimuli; Hunt (10) used a variety of stimuli ranging from colors, photographs, and crimes to objects of art; McGarvey (16) showed verbal meanings are subject to effects of anchors; Pratt (19) and Woodrow (26) have employed psychophysical stimuli and speak in terms of effects of level.

[6] It is apparent that the three-fold classification of field properties into positive, negative, and neutral is adequate only for such simple situations as are discussed here. In white light a large variety of hues may be seen as against the three in monochromatic illumination (illuminant hue, after-image hue, and achromaticity). Similarly, there may be more than one neutral point for complex stimuli, depending upon which aspect or dimension of the object is in question. However, complex stimuli like art objects, advertisements, and pictures of individuals may be judged as wholes and arranged along a single continuum as shown by the work of Hunt referred to above. The difficulty and complexity of the problems which are inherent in more complicated situations than we are dealing with here do not argue against making as good a beginning as we can with material that we can handle experimentally and theoretically.

can thus be said to determine the structure of the behavioral field and the responses which it elicits. If this account of the way the organism functions is correct, then it should be possible to formulate the theory in quantitative terms to yield concrete predictions capable of experimental test. We therefore turn now to this aspect of our problem.

QUANTITATIVE THEORY

In a previous publication (7) it was shown that adaptation-level can be closely approximated by assuming it is a weighted-geometric mean of all stimuli being judged. Formulae have been developed to provide for such factors as area and brightness of visual stimuli, and weight and frequency of presentation of hefted stimuli. The position of the neutral point was successfully predicted in a wide variety of experimental conditions in this way. Here, however, we are concerned with a somewhat different application of the theory since prediction of individual stimuli is in question. We now have to develop a mathematical (psychophysical) function embodying the fundamental assumption that judgments of specific stimuli are made with reference to adaptation-level even when stimuli are explicitly provided to serve as standards or anchors. On this basis, adaptation-level is a parameter in an equation to be determined from observed data by method of least squares. The theoretical values obtained by this method depend therefore only upon the form of the function and the fitted value of adaptation-level. We must now determine the type of function satisfying these conditions and proceed as follows:

If the judgment of a stimulus, X_i, depends upon its distance from adaptation-level, A, it seems reasonable to assume that the judgment is related to the number of just noticeable differences

between A and X_i since 'psychological' distance must be stated in psychophysical terms. We can derive psychological distance from the physical magnitudes involved by dividing the stimulus distance, $X_i - A$, by the value of the j.n.d. in physical units also. According to Weber's law, the increment ΔX which must be added to obtain a just noticeable difference from a standard, X, is a constant fraction of X or:

$$\Delta X = kX, \qquad (1)$$

where k is the Weber constant. According to our basic assumption that judgments are made with respect to A, we should use this value as the base on which the Weber constant k operates. However, since a particular stimulus is being judged at any given time, we need to take its value into account also and hence we take the average of the two as the basis for a j.n.d. from adaptation-level giving equation (2):

$$\Delta A = k(X_i + A)/2. \qquad (2)$$

Equation (2) actually weights the stimulus immediately in perception and adaptation-level equally in determining the size of the j.n.d. and this, the simplest assumption regarding their relative importance, proves to be adequate for a wide variety of data.

We can now write an expression for the number of j.n.d.'s, P, between a variable, X_i, and adaptation-level, A, as follows:

$$P = (X_i - A)/k(X_i + A)/2. \qquad (3)$$

The constants in the denominator of (3) may be replaced by a new constant, K, in the numerator to give:

$$P = K(X_i - A)/(X_i + A). \qquad (4)$$

According to equation (4), when $X_i = A$, $P = 0$. This means a stimulus coinciding with A evokes a neutral or indifferent response and its value fixes the equilibrium point of the organism.

Equation (4) yields a curve which is not symmetric with respect to A. In this respect it differs from both Weber's law and the classical psychometric function of Müller and Urban. The latter has been criticized on this score by Thurstone (24) and others in being at variance with the logarithmic formulation of Fechner. If X's yielding equal P's are stepped off according to Eq. (4), it will be found that the j.n.d. increments become progressively larger in accordance with the Fechner law.

The values yielded by equation (4) are obviously j.n.d.'s above or below adaptation-level and the question arises: "In judging stimuli can, or do, observers report in such terms?" The answer is obviously that they do not and cannot make estimates in j.n.d. steps and if we stopped at this point the usefulness of the theory would be very restricted indeed, not only for this reason but also because we do not know the proper value to assign to the Weber fraction which enters into K in various experimental conditions. *Fortunately equation (4) has certain properties which not only make it unnecessary to know the value of the Weber k but, even more important, allow us to use any numerical scale we please for judging a set of stimuli or for translating qualitative categories into quantitative terms so long as two simple conditions are fulfilled:* (1) K must be the top value of the numerical scale and if a P-value other than zero is to represent the judgment coinciding with the stimulus at adaptation-level its value must be $K/2$; (2) if observers judge in qualitative categories which are to be translated into a numerical scale, the categories must be so chosen and translated as to preserve linearity between the qualitative and quantitative scales. The second condition involves the assumption that observers employ numerical or qualitative scales in linear fashion.

The extent to which they do not do this will worsen the fit to observed data. We shall now show that the *value of* K, *which fixes our numerical scale, can be chosen arbitrarily, and that all* K's *yield identical values of* A and hence it is mathematically permissible to use any scale satisfying the two conditions laid down. The choice of K literally merely determines the scale factor in which the P's are expressed.

To prove that all numerical scales are equivalent that satisfy the two stated conditions we proceed as follows: transform equation (4) into linear form giving (5):

$$(K + P)/(K - P) = X_i/A. \quad (5)$$

Let Q be the uppermost value in a new numerical scale such that $Q = cK$. Then all P values in the Q-scale are multiplied by the same constant so that the new P values are $P' = cP$. To prove:

$$(K + P)/(K - P) = (Q + P')/(Q - P'). \quad (6)$$

We substitute, for Q and P', cK and cP giving:

$$(K + P)/(K - P) = (cK + cP)/(cK - cP) = c(K + P)/c(K - P). \quad (7)$$

which is seen to be an identity. We can therefore choose our numerical scale arbitrarily and do not need to assign any definite value to the Weber constant, k, entering into K.

Returning to equation (5) we note that it is a linear function of the form $Y = mX + b$ where $Y = (K + P)/(K - P)$ and $m = 1/A$, and $b = 0$. Since we shall use (5) to determine the best value of A from observed data by method of least squares let us keep the constant b in order to utilize the added degree of freedom it affords. It is then necessary either to re-define A

or to change the symbol to A' in the new equation which then becomes:

$$(K + P)/(K - P) = X_i/A' + b. \quad (8)$$

The addition of the y-intercept, b, changes the original formulation in equation (4) as re-writing (8) in terms of P shows:

$$P = K(X_i - A' + bA')/ (X_i + A' + bA'). \quad (9)$$

If (9) is solved for $P = 0$, giving the value of X_i evoking the neutral response, or $X_i = A$, we find that:

$$A = A' - bA'. \quad (10)$$

It should be noted that in order to determine A' and b by method of least squares, equation (8) must be used but equation (9) is most convenient for determining theoretical P values after the constants A' and b are known.

So far the development has been in terms of numerical scales such that a stimulus coinciding with A, $X_i = A$, will yield the value zero for P. This means that equations (8) and (9) are in terms of P as deviates from the center of the numerical scale, $K/2$. If we wish to use P values directly in deriving $1/A'$ and b, $K/2$ must be added to equations (4), (8) and (9) so that when $X_i = A$, $P = 0.5K$. We therefore add $0.5K$ to equation (9) and re-write it in linear form which gives:

$$(0.5K + P)/ (1.5K - P) = X_i/A' + b. \quad (11)$$

In determining $1/A'$ and b, our Y values are now the left-hand side of (11). On the other hand, a more convenient form of (11) for solving for P after A' and b are known is:

$$P = K(1.5X_i - 0.5A' + 1.5bA')/(X_i + A' + bA'). \quad (12)$$

TABLE 3

ILLUSTRATING DETERMINATION OF THE CONSTANTS $1/A'$ AND b BY METHOD OF LEAST SQUARES WITH EQUATION (8): $(K + P)/(K - P) = X_i/A' + b$

P_0	$P-50$	Stimuli				P_t	$(P_0-P_t)^2$
		$Y=$ $(100+P)/(100-P)$	X	XY	X^2		
62	+12	1.2727	200	254.54	40,000	62	0
71	21	1.5316	250	382.90	62,500	71	0
79	29	1.8169	300	545.07	90,000	79	0
85	35	2.0769	350	726.92	122,500	85	0
91	41	2.3898	400	955.92	160,000	91	0
		$\Sigma Y=9.0879$ $M_y=1.8176$	$\Sigma X=1500$ $M_x=300$	$\Sigma XY=2865.35$	$\Sigma X^2=475,000$		

$$1/A' = (2865.35 - 2726.37)/(475,000 - 450,000) = 138.98/25,000 = 0.00556$$
$$b = 1.81758 - (0.00556 \times 300) = 0.14988$$
$$A = A' - bA' = 179.89 - 26.96 = 152.93$$
$$P = 100(X_i - 152.93)/(X_i + 206.85)$$

Before proceeding with applications of these formulae it should be noted how the present formulation differs from the commonly used Müller-Urban psychometric function and the Fechner theory.[7] First, observers may employ as wide a range of categories as may be necessary to express the finest shades of judgment or they may judge directly in a numerical scale, thus making it unnecessary to translate qualitative into numerical terms. Second, every qualitative category enters into the derivation of A' and b and hence into theoretical P values. Whereas in the traditional method judgments of 'a little lighter' and 'very, very much lighter' are both indiscriminately counted among the lighter, here they are appropriately weighted and used directly. Third, translation of qualitative judgments into percentages forces data into the symmetrical form demanded by the phi-gamma hypothesis with the result that a number of crucial phenomena in psychophysics remain obscure or

[7] The relation of this theory to other psychophysical formulations, particularly to Guilford's generalized power formula (4), will be discussed in a separate publication.

paradoxical (7, 24). Finally, the present theory differs radically from Fechner's in that judgments are assumed to refer to the equilibrium point which may be far from the threshold stimulus usually taken as the point of origin in the classical law. This makes possible a rational treatment of such 'negative' phenomena as after-images which appear when any part of the visual field is below adaptation-level.

Turning now to the problem of fitting data by method of least squares to equations (8) and (11) we note that the normal equations for the slope and Y-intercept of a linear function (stated in terms of A' and b) are:

$$1/A^i = (\Sigma XY - nM_x M_y)/(\Sigma X^2 - nM_x^2), \quad (13)$$

$$b = M_y - (1/A')M_x, \quad (14)$$

where $X = X_i$, n is the number of values entering into M_x and M_y, $Y = (K + P)/(K - P)$ when P is taken as a deviate from the center of the numerical scale and $Y = (0.5K + P)/(1.5K - P)$ when original P values are fitted. Examples of the application of equations (8) and (11) will now be

given for lifted weight data and judgments of visual mass.

The lifted weight data appear in Table 3 as "P_0" and were obtained as follows: four observers judged weights of 200, 250, 300, 350, and 400 grams as either very, very heavy; very heavy; heavy; medium-heavy; medium; medium-light; light; very light; or very, very light. Two standards of 90 and 133 grams were lifted first in every case followed by the series weights in random order. The qualitative judgments were translated into a numerical scale ranging from 10 for very, very light to 90 for very, very heavy in steps of 10, with 50 representing the neutral or medium judgment. The P values in Table 3 are averages of five series of observations by four Os. The value of K, the top of the numerical scale, is 100 and 50 coincides with the judgment of a stimulus at adaptation-level. From the data and equations in Table 3, the

value of A is seen to be 152.93 and the theoretical P values agree perfectly with the observed. The fact that all judgments are above the neutral point is accounted for by the low value of the neutral point, 152.9, which is below the lightest stimulus in the series, 200 grams.

The values given by the equation for P in Table 3 are actually deviates from the center of the numerical scale, namely, $P_0 - 50$. To obtain actual P values $0.5K$ must be added to the values yielded by the final equation in Table 3.[8] As an example of fitting to P values directly, data on discrimination of color mass published by B. R. Philip will be used with equation (11). These data have been chosen for illustratory purposes because they were obtained under very different conditions

[8] The theoretical values in Table 2 were derived by means of equations (8) and (9) as illustrated in Table 3.

TABLE 4

ILLUSTRATING DERIVATION OF ADAPTATION-LEVEL BY METHOD OF LEAST SQUARES USING EQUATION (11): $(0.5K + P)/(1.5K - P) = X/A' + b$

(Color discrimination data, B. R. Philip, *J. exp. Psychol.*, 1947, **37**, 182)

P_0	$\dfrac{(0.5K+P)}{(1.5K-P)} = Y$	X	XY	X^2	P_t	$(P_0-P_t)^2$
3.32	0.6349	13	8.2537	169	2.45	0.7569
3.66	.6736	14	9.4304	196	3.43	.0529
4.33	.7557	15	11.3355	225	4.30	.0009
4.79	.8168	16	13.0688	256	5.07	.0784
4.83	.8223	17	13.9791	289	5.76	.8649
6.33	1.0566	18	19.0188	324	6.38	.0025
7.00	1.1818	19	22.4542	361	6.94	.0036
7.38	1.2599	20	25.1980	400	7.45	.0049
7.87	1.3692	21	28.7532	441	7.91	.0016
8.32	1.4793	22	32.5446	484	8.34	.0004
9.02	1.6726	23	38.4698	529	8.73	.0841
	$\Sigma Y = 11.7227$ $M_y = 1.0657$	$\Sigma X = 198$ $M_x = 18$	$\Sigma XY = 222.5061$	$\Sigma X^2 = 3674$		

$$1/A' = (222.5061 - 211.0086)/(3674 - 3564) = 0.10452; \quad A' = 9.5673$$
$$b = 1.0657 - 1.8814 = -0.8157$$
$$A = A' - bA' = 17.4$$
$$P = 12(1.5X - 0.5A' + 1.5bA')/(X + A' + bA') = 12(1.5X - 16.49)/(X + 1.7633)$$

from the lifted-weight data just discussed. They represent 'absolute' judgments in that no explicitly given standard was used and the reports were made directly in a numerical scale so that it is not necessary to translate qualitative categories. Eleven cards with dots were presented to observers. To quote from the original description: "In a set of 11 cards the same color combination, e.g., blue-green, appeared in varying amounts on all cards. Starting with the largest number of dots of one color, 23 (and hence with 13 dots of the other color), the number of dots of that color diminished progressively by one, while the number of dots of the other color increased by one" (18, p. 181). Observers judged the number of dots of a given color on a card on a scale ranging from 1 to 11, 1 being indicative of fewest and 11 of the maximum number of dots of a given color. Other details of the procedure can be found in the original article (18). The average scale values have been computed for each card and are given in Table 4 wherein the calculations for determining the theoretical values according to equation (11) are also given. Since A is found to be 17.4, the number of dots of one color reported equal to the other color on the cards is slightly below objective equality, a finding in keeping with results from many psychophysical experiments. The excellence of the fit to these data shows that the mathematical function developed here is adequate not only to gross discriminations, such as the lifted weights with step-intervals of 50 grams, but it is adequate also to fine discriminations as well.[9]

[9] Thurstone has criticized the phi-gamma function as inadequate for gross discriminations though he says it may be good for keen discriminations (24). Our theory also meets his other objection to the phi-gamma function, namely, that it requires a constant limen for all values of stimulus. Equation (8) shows larger and larger stimuli are necessary for equal sense distances.

IMPLICATIONS OF ADAPTATION-LEVEL

Organic function has a double aspect which is at first sight somewhat paradoxical. On the one hand, organisms can respond to such minute amounts of energy as to almost pass belief. Cannon points out the eye is sensitive to 5/1,000,000,000,000 erg, "an amount of energy . . . which is 1/3000 that required to affect the most rapid photographic plate" (3, p. 19). On the other hand, organisms counteract changes in both the external and internal environment to preserve constancy of the fluid matrix and its constituents (homeostasis) in order to maintain life. We thus find remarkable stability coupled with extraordinary capacity for discrimination. In the words of Charles Richet, quoted by Cannon (3, p. 21), "By an apparent contradiction it (the body) maintains its stability only if it is excitable and capable of modifying itself . . . and adjusting its response to . . . stimulation." How these two apparently antithetical properties can exist simultaneously appears in a new light from our point of view. We have seen how the point of equilibrium shifts with changes in background but does not move so far as to completely counteract extreme stimuli. In monochromatic light the hue of the illuminant is still visible, though reduced in saturation; similarly, when a very heavy standard is employed some of the series stimuli are still judged as heavy. In spite of reduction in sensitivity to dominant parts of the field, the organism is able to appreciate and to discriminate their qualities because adaptation does not completely negate extreme stimuli. While the organism is adapted to specific parts of the stimulus-continuum it is sensitized to stimuli some distance away. Adaptation to red makes us more sensitive to blue-green; a heavy standard predisposes to judgments of light; and affliction makes us readier to

smile at trifles. Increased sensitivity to less-adapted parts of the field places the organism in readiness for new stimulation and balances the field dynamically. An equilibrium requires a balance of opposing forces with the result that the field is qualitatively structured with as little as possible missing. Both qualitative and dynamic considerations therefore require such a theory as is here proposed.

Having discussed some of the more general features of organic functioning in the light of the theory of adaptation-level we now briefly consider some of its specific implications since the ultimate test of any theory is its ability to handle concrete problems. The most pressing and crucial problems in the world today concern human relations and there is considerable pressure to apply whatever comes to hand from all branches of psychology. Concepts of frames of reference, values, and norms have been carried over into social psychology from psychophysics but unless they are properly founded with respect to basic considerations their applicability is strictly limited. On the other hand, clarification of basic problems leads to new research and may re-structure thinking so that applications can be made more fruitfully. With these considerations in mind let us see how some of the fundamental problems concerning frames of reference are re-oriented in the light of the present approach.

(1) *Shifts in neutral point.* That the neutral region, or point of subjective equality, varies has been known for a long time and has been regarded as an indicator of changes in organic functioning. What has not been recognized is that the behavioral field undergoes re-structuration when shifts in adaptation-level occur. Often these shifts have been regarded as mere changes in 'scale of judgment' whereas

their true significance lies in an entirely different direction. Not only is the phenomenal field altered but responses to objects also change, often in radical fashion. Thus one observer reported that it was much less fatiguing to use the 900 gram standard than to judge the stimuli individually "because the weights are so much lighter now" although the total weight lifted with the standard was 4500 grams greater per series than in the single method! The after-image hues seen in strongly chromatic illuminants in the darker parts of the field are as saturated, persistent, impelling, and 'real' as any colors can possibly be. The theory of adaptation-level gives a rational account of negative as well as positive gradients and recognizes their importance in the economy of the organism. Anything as striking and universal as the phenomena associated with changes in adaptation-level must be rooted in basic mechanisms and hence must be characteristic of organic functioning in general. The theory here proposed emphasizes the basic fact that shifts in the neutral point are accompanied by re-structuration of the entire behavioral field and are not mere shifts in point of subjective equality or the indifference point.

(2) Traditional and current theories of the part played by standards in the phenomena we have been discussing are too simple to fit many facts. Because a response follows after presentation of standard and a variable, it has been assumed that only these stimuli form the basis of the judgment plus special contrast or assimilation effects. Certain crucial facts unexplained by this assumption have been covered up in the classical methods which allow only a restricted range of responses, such as greater, equal, less, and cast the results into percentage form. We have already pointed out above that it is, to say the least, anomalous on the basis of con-

trast and classical theory for a variable less than half as heavy as the standard to be called 'medium-heavy.' Only if the judgment is based on something *lower* than the variable, namely, adaptation-level, can this result be satisfactorily explained.

In taking the neutral point rather than so-called standards as the point of reference we can explain why anchors exert as much effect as they do; however this effect amounts to less than would be expected on the theory that a judgment expresses only the specific relation between standard and variable. The effect of the standard, unless the conditions of experimentation artificially force judgments to be specifically directed to a simple, paired comparison, is indirect and only by way of its effect on adaptation-level. Were this not the case the formulae given above would not fit observed data so well since they embody the assumption that judgment of a stimulus depends upon its relation to adaptation-level rather than to the standard.

Closely related to the problem of the effect exerted by anchoring stimuli are two important findings which the present theory explains satisfactorily also. The first is that a given stimulus, which may be one of the series or far above or below, may be used as an anchor or presented as one of the series. What difference does it make whether a stimulus is employed as standard or as series member? The difference is strikingly brought out if a stimulus far outside the series range is tested. In a previous publication (7) it was shown that a stimulus exerts greater effect as anchor than as member of series because it is presented more times as anchor and thereby makes a greater contribution in the determination of adaptation-level. But the fact that the effects of series stimuli could be shown operative in determining adaptation-

level supports our contention that it is the pooled effect of *all* stimuli, series and standards, which determines the judgment.

The second finding which the present formulation accounts for concerns certain special cases when the relative and 'absolute' methods yield identical results. According to our fundamental hypothesis, judgments of stimuli should be the same, whether or not a standard is employed, if adaptation-level has the same value in the two methods of judging. On the basis of a quantitative theory of adaptation-level it was shown that the neutral point in the method of single judgment lies somewhat below the logarithmic mean of the series. Experiments with the two methods gave almost identical results when the standard was so chosen in the comparative method that the value of adaptation-level was the same as in the method of single judgment (7). The present theory thus bridges the gap between the comparative and 'absolute' methods.

(3) *Contrast, central tendency, and repellent effects.* Current views of these phenomena are in the empirical stage devoid of any unifying theory. The tendency to avoid extremes of judgment, first pointed out by Hollingworth (9), has been regarded as opposed to the contrast effect between standard and variables (15). That central tendency is a special case, found only when method of single judgment is employed or when the standard lies within the series, has not been recognized. *If standards far enough above or below the series are employed, the shifts in judgment will be found to be not toward the center of the series but toward the stimulus coinciding with adaptation-level, wherever it may be.* This statement of the facts is both more general and more correct than the law of central tendency and is in accord with the shape of the function embodied in equa-

tions (9) and (12). This function rises rapidly below adaptation-level and more slowly above. This means stimuli below adaptation-level are judged relatively higher while those above are judged relatively lower than they would be if the function were linear or had a positive slope.

The repellent effect of so-called standards, which is closely related both to contrast effects and shifts toward adaptation-level, is explained by the position of the neutral region. High standards pull adaptation-level up and thereby force stimulus-values downward while low standards have the opposite effect by lowering adaptation-level. Both contrast and repellent effects thus depend upon the relation of any stimulus to adaptation-level. It is suggestive to think of stimulus as a physical quantity remaining fixed while adaptation-level moves up or down the stimulus-continuum, depending upon the total context of conditions, making the *effective stimulus* now positive, now negative, and sometimes neutral so far as the organism is concerned. Stimulus properties therefore depend upon the state of the organism which, in turn, is determined by the total stimulus-organic configuration. The quantitative as well as qualitative clue to the structure of this configuration is the position of the neutral region.

(4) *Applications to social behavior.* Our discussion so far has been directed toward clarification of problems in perception and judgment of more or less simple, objectively measureable stimuli. It may be asked if the theory can be fruitfully used for the investigation of more complex forms of behavior. While applications of any theory are extraneous, in a sense, to its validity, and are a matter for specialists in the various fields, it is legitimate to raise this question since the value of a theory lies, in part, in its generality. Let us therefore turn to an experimental approach to some problems in social behavior embodying some of the essential points of adaptation-level theory. Preston and Baratta (20) devised a game in which subjects, working in groups of two or four, bid for the privilege of winning prizes ranging from five to 1000 dollars (in 'paper' money), knowing the probabilities of obtaining each prize. Dice were thrown and if the winning combination appeared the highest bidder received the reward. The objective value or expectation, E, of the prize is equal to its value, V, multiplied by the probability, p, of winning it, or, $E = Vp$. If the psychological expectation, regarded as the highest bid, is equal to the objective expectation, a plot of the two should be linear. Results from a large number of experiments involving prizes ranging from 5 to 1000 and probabilities of winning from .01 to .99, hence with objective expectations ranging from .05 to 990.00, show that, both for naive and mathematically sophisticated subjects, "for all values of prize from 5 to 1000, the mean winning bid exceeds the mathematical expectation for small values of the probability and is less than the mathematical expectation for large values of the probability" (20). The plot of the bets must therefore pass through an indifference point which coincides with the objective expectation. As the authors of the study point out, the indifference point is somewhat less than .25, a value near the logarithmic mean of the probabilities and very close to the value of adaptation-level derived by the writer for visual, kinaesthetic, and sound data (7). Further implications of adaptation-level theory will be found in the study by Preston and Baratta having to do with group behavior. It is evident from this study that the concept of adaptation-level does lend itself to problems in social behavior.

Some broader implications of the theory proposed here for both individual and group behavior merit consideration even though they are so general as to fall within the category of almost pure speculation at this time. These concern the problems of general attitudes and traits, the phenomenon of 'social lag,' and the mechanisms responsible for revolutionary changes in social structures.

Owing to the difficulty of conceiving how specific responses can be referred to general mechanisms, many psychologists have rejected the notion of generalized traits and attitudes. On the other hand, it is just as difficult to conceive of responses as wholly specific no matter in what sense this is taken. The difficulties can be largely resolved by proper definition and re-orientation of the problem. We usually mean by a general attitude outright acceptance or rejection of propositions, people, or objects, without regard to the differences among them. Responses of this kind having to do with social, moral, and aesthetic objects are paralleled in the field of perception under certain conditions. In monochromatic illumination all objects on a daylight white background have the hue complementary to the illuminant hue and on black background they all have the hue of the illuminant. Extremely high or low reflectance of background rules out certain types of response, accentuates others. Figuratively we can say that in monochromatic illumination strong biases are evident. A background of intermediate reflectance yields the largest variety of qualities, as seen from Table 1 above, and so favors discrimination or what may be called a 'liberal attitude.' The problem of general attitudes and traits can thus be re-formulated in terms of the measure of discrimination shown, the number of individuals accepted or rejected in a given category, the condi-

tions making for acceptance or rejection, and in terms of whatever other characteristics of behavior are relevant. A generalized trait of honesty would mean the neutral point for acceptance of acts as honest is so high that the slightest semblance of dishonesty is rejected; conversely, dishonesty could be gauged by the extent to which certain acts are accepted. An individual's neutral point can thus furnish the clue to attitudes, traits and personality structure.

On the basis of our theory, the actions of groups can be fruitfully regarded as resultants of individual attitudes, hopes, desires, opinions, and responses which are more or less pooled in a manner not unlike the pooled effects of a series of weights and standards.[10] Two striking features of group behavior, at opposite ends of the continuum of group dynamics, call for explanation. The phenomenon of social lag, or the tendency for group action to be slow, along certain lines, furnishes good evidence for a *social level* which moves more slowly than many of the individuals comprising the group, just as adaptation-level exhibits inertia in the face of extreme stimuli. Such precipitates of social behavior as laws, religion, taboos, and rites represent the 'adaptation-level' of a society insofar as they are typical of a people. The fact that many individuals do not act in conformity with socially accepted standards while overtly or covertly opposing them points to the difference between individual and group levels. Adaptation-level as a weighted mean of all factors entering into group behavior provides a key to many phenomena attributed to social lag.

Groups do not, however, always move

[10] Tresselt and Volkmann (25) have shown that individuals tend toward uniform opinion not only by being subjected to restricted ranges of social stimulation but also to restricted ranges of physical stimulation.

slowly as is shown by the actions of mobs, the violent changes accompanying revolutions, and the rapid but peaceful transitions in our own society. While violent changes may be the culmination of long build-up, phenotypically they may appear with startling suddenness. Great leaders, extreme economic conditions, persistent propaganda may raise or lower mass levels of acceptance or rejection of ideas to the point where new patterns of action emerge in society. The rate of social change would correlate, on our hypothesis, with the rate of change of level, while the violence of the change would depend upon the extent of change in group level.

SUMMARY

The concept of adaptation-level is shown to be capable of quantitative formulation and thereby provides a much-needed approach to frames of reference, norms, and related phenomena. A mathematical theory embodying the assumption that the structure of configurations depends upon the position of the neutral point yielded formulae which make possible determination of adaptation-level and predictions of individual stimuli from experimental data. The excellent agreement between calculated and observed values indicates the psychophysical function developed here is adequate to deal with a large variety of data obtained under various experimental conditions and is applicable to gross as well as fine discriminations. The theory is shown to be fruitful in suggesting experiments and in interpreting certain aspects of individual and group behavior.

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PERCEPTION UNDER STRESS

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Perceiving is goal-directed behavior. The goal of perception, in its broadest sense, is the construction of a meaningful behavioral environment—an environment congruent with 'reality' on the one hand and the needs and dispositions of the organism on the other (2, 3, 4, 5, 8, 9). If perceiving is indeed considered a form of goal-directed behavior, the frustration of perceptual processes should have predictable consequences. The consequences of frustration are, to be sure, manifold (10). But one important consequence, highly predictable in the light of recent research, is the disruption of behavior following upon the thwarting of goal-directed activity—behavioral regression or primitivation (11). We may properly inquire, then, whether the frustration of efforts to perceive leads to a primitivation of perceptual organization.

Before inquiring into the nature of the effects to be expected from perceptual frustration, let us first examine in some detail those processes involved in perception which might be most liable to disruption. In order to achieve meaning, the organism must select its percepts from a multitude of potential stimulus configurations, emphasize and vivify them, and render them stable and coherent in the face of continuing sensory flux (2). Through this threefold process of *selection, accentuation*, and *fixation* the organism strikes a balance or compromise in his perception between the requirements created by physical, biological, and social existence. He must locomote in physical space while satisfying often imperious psychological needs in ways prescribed by society. To construct his perceptual world man requires an elaborate sequence of *hypotheses* through which he seeks out, accepts, and rejects what is perceptually demanded by his needs, by his reality situation, and by his past experience. Hypothesis, as we use the term, denotes the attempt to perceive a stimulus configuration in a certain way, for a given purpose, or with a given meaning. Man is perpetually prepared or *eingestellt* in one way or another and what he sees at any moment is a resultant of this preparedness and of the nature of the stimulation bombarding him.[1] To some stimuli he becomes sensitized, to others virtually oblivious. He learns to eliminate from his perceptual field what is extraneous to him and to encompass what is important even to the extent of occasionally 'seeing things that aren't there.' In a very real sense perception is a first line of defense against would-be catastrophic situations and a sensitizer to adaptive opportunities.

[1] The reader may properly ask at this juncture, "What is the difference between a perception and an hypothesis?" We should have liked to dispense entirely with the latter term, *hypothesis*, subsuming it if possible under the rubric *perception*. Perception, however, has come to mean the phenomenal resultant of the perceiving process, and only the resultant. A perception is an experience of something. Its connotation, indeed its denotation, is passive: a picture or an image or a content of consciousness. Hypothesis is the short-hand term we are using to describe the pattern of acts which go into the process of perceiving. Had we been more reckless of English usage, we might have used in place of the term, 'an hypothesis,' the expression 'a perceiving.'

Like all goal-seeking activity, perceptual behavior is sometimes blocked by obstacles—one fails to perceive, for one reason or another, what was necessary or important in a situation and thereby fails to adjust. Inevitably, there is perceptual frustration just as there is frustration of other instrumental and consummatory acts. In the face of thwarting, the organism's energies may be canalized into either need-persistive and adjustive attempts to get to his goal (1, 10) or behavior may be subordinated to the service of direct or displaced aggression (6) or simply 'go to pieces' (11). Are comparable reactions to be found when perception suffers frustration? The organism may show reactions in several spheres. His capacities for selecting adaptive percepts may suffer. He may, and there is evidence that he does (3), accentuate his perceptual field differently. The sequence of hypotheses so necessary in perceiving new stimulus situations may, finally, be seriously disrupted. A man, frustrated in his efforts to 'find' the traditional and elusive collar button under the bureau, may too readily see specks of dust or paper as the sought-after object. He may, moreover, persist too fixedly in seeing the specks as collar buttons to the point where he must touch each one to be sure. Or, as frustration mounts, his capacity for examining or selecting clues deteriorates to that level at which his wife intercedes with the usual, "Why, there it is right under your nose, Dick."

Such changes in perceptual behavior may well be subsumed under the heading of *primitivation*. Inadequate selection, unrealistic accentuation, and erratic fixation characterize perceptual reactions which fall short of optimum adaptation to the environment. Measured with the yardstick of behavioral utility, they represent a more *primitive* level of function than is usually observed in the absence of frustration. Our example, drawn from casual observation, is scarcely exhaustive. The aim of the research reported in the pages that follow is to achieve a more systematic understanding of what follows perceptual frustration and in what manner.

THE EXPERIMENT

Five control and five experimental subjects took part in the experiment. The stimulus materials for all subjects consisted of eighteen three-word sentences, presented one at a time in a modified Dodge tachistoscope. Subjects were instructed to report everything they saw or thought they saw.

The method of presentation used was a modification of the method of limits. Each sentence was first presented once at .03 second below threshold, then once at .02 below, twice at .01 below, twice at threshold, twice at .01 above, and once at .01 second intervals beyond that until all three words were correctly recognized.[2] As subjects learned, their threshold exposure level occasionally had to be revised downward by .01–.03 second. This was done when two words were perceived correctly on the first trial at .03 second below their previously established threshold. A full record was kept of each response, correct or incorrect, given by the subject at each exposure.

The procedure on the first nine sentences (Initial Series) was the same for the Experimental and Control Groups: subjects were simply required to continue their judgments until they had recognized each sentence in turn. At

[2] In a preliminary test the subject's threshold was determined by increasing exposure time from a starting level of .05 second in steps of .01 second until the subject saw one of the three words in a test sentence correctly. The exposure level thus obtained was taken as a rough measure of threshold upon which the procedure in later trials were based.

the end of these trials, Experimental subjects were placed in a perceptually frustrating situation. Each Experimental subject was told that he was to be shown in the tachistoscope 'something' which he was to describe to the experimenters as fully as possible. Instructions were given in a serious tone of voice by the two experimenters, the authors. A highly complex picture, reproduction of a crowded painting in black and white, was then presented to the subject at low illumination and at a speed of .01 second.[3] The initial length of exposure and the illumination were at a level at which the subject was unable to make out anything save the vaguest contours and shadings. Upon reporting that he was unable to see very much at all, the subject was told by one of the experimenters in rather icy tones that it was of the utmost importance that he pay very close attention to instructions if the study was to be effectively done. He was told to try again and informed that his performance was substandard. Since the subject understood that the experiment dealt with visual acuity, he could have no inkling (and none did, according to their reports) that the experimenters were interested in anything other than eliciting his closer attention. On successive presentations of the picture, the subject invariably began to make more and more guesses. These would be greeted with such remarks as, "Are you sure you are reporting frankly what you see? You needn't be embarrassed to tell us everything that is there." During ten to twelve exposures of the picture the subject was constantly badgered as to whether he "hadn't seen something more." On occasion, the experimenters told the subjects that the interval of ex-

posure was to be lengthened. Generally the interval was not increased, but sometimes, if the subject was not seeing enough to make the task provocative, the interval was increased slightly. At one point during the frustration period, the experimenters would ask whether they could look more closely at the subject's eyes. This they would do with a certain amount of technical double-talk to each other about, for example, "the aqueous and vitreous looking clear enough." The subject was repeatedly told that he was apparently not doing as well as he should be doing on the basis of his record during the Initial Series. Various questions were asked about his state of mind: whether he was feeling rested, whether he had experienced any recent emotional upset, whether he had been having "incomplete dreams in which he found himself chasing things and not catching up with them." The treatment was admittedly harsh, but it was effective. Subjects grew tense, were obviously trying too hard, began swearing under their breath or out loud and were quite strikingly frustrated. Perception of the picture became for them the way out of an unpleasant and embarrassing situation. Looked at socially, here were our subjects—some of them Harvard graduate students or advanced undergraduates or the wives of Harvard graduate students, but none of them sceptical students of psychology—failing in a task assigned them by two Harvard faculty members who were insistent about their performance beyond the point of rudeness. That they were feeling frustrated and thwarted was clear not only from their overt behavior but from the testimony given after the experiment was over and its purpose explained to the subjects.

The Control subjects received a totally different and much more pleasant treatment at the end of their Initial Series. They were shown the same picture

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[3] The picture, reproduced in the dimensions 4½″ × 6″, is the 'Tenement Scene' by the American artist, Billings. It depicts a complex cross-section of tenement life in three panels, each representing a busy and crowded scene.

exposed to the Experimental subjects, but for a period of 30 seconds. During and after this exposure they were asked to report in fullest detail what they had seen in the picture. Following this, the subjects were shown the picture again at two very obviously different levels of illumination. They were told to bear these illumination differences in mind since they would be asked again to distinguish between them. This interpolated series was used to equate the Experimental and Control subjects on all but one variable—frustration. Again, the situation created was one of testing vision.

The Test Series was alike in all but one crucial respect for the Experimental and Control groups. Each received nine more three-word sentences for recognition. For members of the Control group five exposures of the picture were interspersed at random among the ten sentence presentations. These subjects were asked upon exposure of the picture whether it was at high or low illumination. Few mistakes were made and whether the subject was correct or not, he was told that he had made the right judgment. The Experimental subjects received five presentations of the picture at .01 second interspersed among

TABLE 1

SUMMARY OF EXPERIMENTAL DESIGN

Series	Experimental Group	Control Group
Initial Series	9 three-word sentences: threshold determination	9 three-word sentences: threshold determination
Interpolation Period	Frustration in perception of picture	Full exposure of picture
Test Series	9 three-word sentences: threshold determination. 5 interspersed exposures of picture with frustration of perception.	9 three-word sentences: threshold determination. 5 interspersed exposures of picture with successful illumination judgments.

TABLE 2

STIMULUS SENTENCES

Initial Series: Warn bold liars (Preliminary threshold determination sentence)
Church shuts early
Books grow dull
Heroes also died
Time has healed
Figures tell fact
Money buys hate
Truth serves fancy
Beauty will perish
Banks desire cash

Test Series: Stop foul crime
Praise hardy deeds
Tests show much
Armies lack hope
Help was sacred
Science must halt
Power pays much
Peace hardly lasts
Swords rust slowly

the sentences, again with the urgent instruction to tell everything they had seen. Their reports were greeted with sighs, groans, or expressions of puzzlement by the experimenters. "We don't understand why you can't do better with the picture," was the theme around which the experimenters' reactions were built during these picture presentations. In the case of sentences, no comments, favorable or otherwise, were offered by the experimenters either during the Initial or Test Series. The design of the experiment is briefly summarized in Table 1, the sentences used in testing in Table 2.

THE RESULTS

The critical test of our hypothesis consists in the comparison of the perceptual behavior of the Experimental group and the Control group during the Test Series. The Experimental group has suffered perceptual frustration prior to the Test Series and continues to undergo frustration intermittently during this series. The Control group, on the other hand, has been unthwarted

throughout. What kind of changes can be expected in our subjects' thresholds; to what extent are there alterations in the frequency and quality of hypotheses preceding correct recognition of the sentences?

Threshold changes: Three threshold measures were obtained: (a) the exposure time required for the correct recognition of all three words in a stimulus sentence—the *sentence threshold;* (b) the exposure time required for recognition of two of the three words— the *two-word threshold;* and (c) the time required for recognition of one of the three words—the *one-word threshold.*

When sentence thresholds in the Initial and Test Series are compared, we find that the Control group's threshold has decreased in magnitude as a function of learning, while the threshold of the Experimental group remains virtually constant.[4] *As far as sentence thresholds are concerned, then, frustration prevents experimental subjects from improving as much as they might have with practice.*

At what level of perceptual performance do the effects of frustration manifest themselves? Do they affect only perception of whole sentences or is the process of building up sentences word by word also hindered? Analysis of the two-word threshold reveals again that the Control group improves considerably from Initial to Test Series while the Ex-

[4] The average thresholds of the two groups for whole stimulus sentences in Initial and Test Series are presented in Table 3. It is clear that the thresholds throughout the experiment depend upon the visual acuity of the individual subjects. To make fair estimate of the effect of experimental treatment, it is necessary to compare the *distributions of differences* between Initial and Test Series for the two groups. When the correlation between Initial Series and Test Series is thus taken into consideration, the mean difference between the two groups yields a *t*-value of 2.77, with a *P* slightly above .02.

TABLE 3

AVERAGE EXPOSURE TIMES (IN SECONDS) REQUIRED FOR CORRECT RECOGNITION OF 1, 2, AND 3 STIMULUS WORDS

	Experimental Group		Control Group	
	Initial Series	Test Series	Initial Series	Test Series
One word correct	.115	.112	.106	.092
Two words correct	.132	.133	.122	.104
Three words correct	.154	.152	.136	.116

perimental group remains virtually at a standstill (Table 3). *The effects of frustration, then, are manifest in the building up of the perception as well as in its final phase.* Although not highly significant, this difference satisfies reasonable criteria of statistical reliability, *t* being 1.82 and *P*, .10.

How far-reaching were the effects of frustration on the perceptual behavior of our subjects can be seen from the fact that the *one-word thresholds* of the two groups also diverge in the Test Series. Again we find the members of the Control group showing an improvement and the performance of the Experimental subjects practically unchanged (Table 3). The data here are suggestive but fall short of statistical significance ($t = 1.31$, $P = .20-.30$). *Frustration, then, affects not only the ability to perceive a complex stimulus in its entirety but the capacity to structure less complex components as well.*

The measurement of sheer threshold times reveals only one aspect of the manifold changes resulting from frustration. Nor do the differences in threshold exposure time provide the most striking consequence of perceptual frustration. It is through analysis of the subjects' pre-recognition behavior —the nature and pattern of the hypotheses which precede full recognition of the sentences—that the full impact of frustration can best be gauged.

Deterioration of hypothesis control: For optimal perceptual adjustment in a shifting, ambiguous, or otherwise difficult perceptual situation (such as our experiment) a nicely balanced flow of hypotheses would seem to be essential, one which is neither restrictively slow nor disruptively fast. Hypotheses need to be continually tested in action and accepted or rejected neither too early nor too late but, rather, according to their success or failure in the creation of adequate meaning. We might well predict that frustration would interfere with a nicely balanced hypothesis flow, causing either a restrictive slow-down or disruptive speed-up. In our experimental situation, such a deterioration of hypothesis behavior can be measured in terms of the relative frequency and patterning of a subject's hypotheses from one stimulus exposure to the next. On any one exposure, the subject could see, correctly or incorrectly, *all* of the three stimulus words in a sentence, *two* of them, *one* of them, or none of them at all. By *seeing* a word we mean not merely sensing that some letters were spread out in a line on the stimulus card. To see a word means to see it well enough to have an hypothesis as to what it is. Recall that subjects were instructed to report "everything you see or think you see." We refer to a correct or incorrect perception of three words as a *three-word hypothesis,* to the perception of two of them as a *two-word hypothesis,* and of one of them as a *one-word hypothesis.* Where the subject saw no words we refer to his behavior as a *blank.* For all exposures of all words, the frequency of one-, two-, and three-word hypotheses were recorded as well as the incidence of blanks.

The prediction that hypothesis patterns change as a result of frustration can be verified by a comparison of the relative frequency of hypotheses falling in the four categories during the Initial and Test Series. This comparison was carried out by means of a contingency test for the differential association of hypothesis categories with the two se-

TABLE 4

CONTINGENCY TEST FOR DIFFERENTIAL ASSOCIATION OF HYPOTHESIS CATEGORIES WITH INITIAL AND TEST SERIES

Numbers in parentheses represent theoretical frequencies; the others are obtained frequencies

	Experimental Group			Control Group		
	Initial Series	Test Series	Total	Initial Series	Test Series	Total
3-word hypotheses	67 (89.5)	124 (101.5)	191	113 (112.3)	109 (109.7)	222
2-word hypotheses	83 (71.7)	70 (81.3)	153	61 (62.2)	62 (60.8)	123
1-word hypotheses	166 (156.5)	168 (177.5)	334	64 (62.7)	60 (61.3)	124
Blanks	34 (32.3)	35 (36.7)	69	27 (27.8)	28 (27.2)	55
	350	397	747	265	259	524

$$\chi^2 = 16.22 \qquad\qquad \chi^2 = .07$$
$$P < .01 \qquad\qquad\qquad P > .99$$

ries. The test clearly shows that the distribution of hypotheses undergoes no change whatsoever from Initial to Test Series in the case of the Control group (Table 4). The odds are more than 99 in 100 that the minuscule differences between the two series could have been obtained by chance. The experimental group, on the other hand, presents a strikingly different picture. A very highly reliable change in the relative frequency of the different kinds of hypotheses follows upon frustration. The chief characteristic of this change is in the direction of greater incidence of full sentence (three-word) hypotheses. Under the pressure to prove oneself in the face of repeated failure, hypotheses become reckless. The subject tries to see full sentences prematurely. This increase in prematurely complete attempts at recognition is not, as the threshold measures show, accompanied by any corresponding increase in correct perceptions. On many occasions, these three-word hypotheses made neither grammatical nor any other kind of sense, e.g., *Th.. shoo rough* for *Tests show much* or *What money deeds* for *Praise hardy deeds*.

We have here an important difference between performance under stress and performance under normal conditions. Under normal conditions, the final recognition is usually preceded by a period of systematic exploration, and the first three-word hypothesis is, more often than not, fairly close to fact. Under stress, much of the systematic exploration is short-circuited, and responses are triggered off as soon as the subject has a minimum of perceptual material to work with. Once having formed a perception on the basis of a premature hypothesis, the subject must then 'un-perceive' it—an act accomplished only with considerable difficulty (7).

Our interpretation of the increase in three-word responses as an expression of premature hypothesis formulation also helps explain the decrease in two-word hypotheses following frustration (Table 4). Such two-word hypotheses represent a crucial exploratory step before final recognition, a step which the frustrated subject fails to take. The relative frequency of one-word hypotheses and blanks, on the other hand, is not seriously affected. The subject could not, to be sure, use one-word hypotheses for the successful full-sentence recognitions he wanted to make in order to overcome his failure on the picture. Such one-word hypotheses and blanks are, moreover, simpler perceptual acts and, therefore, less liable to disruption under stress. Frustration, unless it be pushed to an experimentally prohibitive extreme, is not likely to affect one's capacity to distinguish the presence from the absence of a stimulus. An illustration from everyday life clarifies the point. There are few perceptual experiences more frustrating than a crucial but poorly received long-distance telephone message. In such a situation perceptual organization may become primitivized or disrupted and frustration may leave us even less capable of hearing what is being said. One of the first things to suffer, perhaps, is the capacity to perceive and understand complex word groupings and their connotative nuances. The last thing to be affected is the ability to catch single words and to discriminate speech from random noise.

Frustration and the content of hypotheses: Frustration should also affect the content of hypotheses. Frustration theory predicts more aggressive and escape responses during and after stress. Such was indeed the case. A careful inspection of all records showed the following distribution of these aggression

and escape words reported as seen by our subjects.[5]

	Initial Series		Test Series	
	Aggression words	Escape words	Aggression words	Escape words
Experimental Group	2	0	17	6
Control Group	1	0	1	0

It goes without saying that a simple explanation of the perception of aggression and escape words in terms of their resemblance to the structure of the stimulus words does not suffice since both Experimental group and Control group were exposed to the same stimulus materials. Many of these responses, moreover, bore no resemblance whatever to the structure of the stimuli.

Some examples of aggressive and escape responses given by the experimental subjects during the Test Series are instructive: *Keep silent* for *swords rust* (withdrawal or escape reaction), *burst* for *rust, perish* for *praise, foolish hard doing* for *praise hardy deeds, treat . . . rough* for *tests . . . much, screamed* for *sacred*, and *hurt* for *rust*.

The increase of aggression and escape words following frustration illustrates a principle of perceptual behavior which we have discussed in a somewhat different context elsewhere—*perceptual resonance*. In a study of personal values as selective factors in perception (9) it was found that as frequently as possible and for as long as possible in a tachistoscope situation, the subject produces on successive exposures perceptual hypotheses which represent his personal values. When stimulus words reflecting these

values are presented in the tachistoscope, the subject recognizes them more rapidly than other, irrelevant words. When, in short, there are objects in the environment congruent with his value-oriented hypotheses, his perception is aided by resonance. Should such appropriate objects be absent, his perception is hindered and 'led astray.' Such was the case in our present experiment. In the Test Series, the frustrated subject hypothesized aggression and escape words but there were no fitting stimulus words. Resonance could not occur. His hypotheses were abortive in achieving sought for meanings. Had the Test Series included words symbolizing aggression, the frustrated subject would very likely have seen them sooner than the calmer control. A token of this 'postdiction' is provided by the rash of violent responses to the stimulus word *rust* ringing the changes on *bust, burst*, and *hurt*. Suppose the stimulus word had actually been *bust?*

Perception as goal-directed behavior: The changes in the behavior of our Experimental subjects during the Test Series—failure to benefit from practice, increase in premature and reckless hypotheses, and the intrusion of aggression and escape hypotheses in the absence of appropriate stimuli—have all the hallmarks of reactions to thwarted goal striving. They are, moreover, changes which are highly congruent with each other, leading to a primitivation or disruption of function in the main spheres of perceptual behavior. The ability to *select* percepts from a complex stimulus field suffers: thresholds remain high and the line of demarcation between sense and nonsense is blurred. The degree of subjective *accentuation* becomes maladaptive in its emphasis on aggressive and escape interpretations of the stimuli. Finally, there is premature and unwarranted *fixation* of hypotheses as interpretations of the stimuli are produced

5 While only two judges analyzed the records, most of the aggression or escape words were quite unequivocal, and a more elaborate method of checking the reliability of our classification was thought unnecessary.

recklessly without sufficient exploration and test. These are the main dimensions of perceptual primitivation under stress.

SUMMARY

The goal of perception, in its broadest sense, is the construction of a stable and meaningful environment. What are the consequences for perceptual organization when this basic goal is thwarted? We have attempted to answer this question in the preceding pages.

Two groups of subjects (an Experimental group and a Control group) were required to recognize three-word sentences presented to them in a modified Dodge tachistoscope. After an Initial Series which was identical for the two groups, the Experimental group was subjected to perceptual frustration, being given the impossible task of describing a picture which was presented tachistoscopically at subthreshold speeds. Their failure was intensified by bitingly disruptive criticisms from the experimenters. The Control subjects were shown the same picture at fully adequate exposure times and successfully performed an easy judgment task. In the Test Series three-word sentences were again presented, interspersed with further presentations of the picture. When the performance of the two groups on the Test Series is compared, striking perceptual changes consequent upon frustration make their appearance. Unlike the Control subjects, the Experimental subjects fail to benefit from practice, and their thresholds for recognition of the sentences do not change. The adverse effects of frustration extend to the perception of components of the total stimulus (single words). Under the pressure of continued failure perceptual behavior becomes reckless: premature and often nonsensical interpretations of the stimuli are made. The content of the hypotheses preceding full recognition also changes: frustration leads to a considerable increase in responses reflecting aggression and the need to escape the situation.

These consequences of frustration—and here we must caution that they do not represent an exhaustive catalogue of the consequences of frustration—may be subsumed under the heading of *primitivation*. Perceptual behavior is disrupted, becomes less well controlled than under normal conditions, and hence is less adaptive. The major dimensions of perceptual function are affected: selection of percepts from a complex field becomes less adequate and sense is less well differentiated from nonsense; there is maladaptive accentuation in the direction of aggression and escape; untested hypotheses are fixated recklessly. Perception, which is indeed goal-directed behavior, is dramatically changed under the impact of frustration.

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LEEPER'S 'MOTIVATIONAL THEORY OF EMOTION'

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In a recent article in the PSYCHO-LOGICAL REVIEW entitled "A Motivational Theory of Emotion to Replace 'Emotion as Disorganized Response,'" Dr. Robert W. Leeper attacks the common textbook definition of emotion as disorganized response and offers in its stead "a motivational theory of emotion" (7). Since I myself have long been interested in theories of emotion, and, following an initial venture in 1932 (2), have written a number of papers on the subject, I find myself impelled to comment on this recent contribution.

I am happy indeed to have so able an ally as Dr. Leeper in the fight against current theories of emotion—even though Dr. Leeper is apparently unaware that he is not waging the battle alone. I have, however, one serious objection to Dr. Leeper's attack: *it does not go far enough*. He directs his guns at only *one* of the current types of definition of emotion—that of emotion as disorganized response. Other varieties of definition, equally vulnerable, are left unscathed. And, finally, Dr. Leeper offers his own definition of emotion which, in the form in which it is stated, is itself untenable.

In 1934, in an article in this journal entitled "Emotion: An Example of the Need for Reorientation in Psychology," I examined the various types of definition of emotion offered by psychologists and concluded that attempts had been made to differentiate emotion from other states or conditions on five bases (3). These bases, as presented at that time, were as follows:

1. The physiological mechanisms involved in the response, *e.g.*, visceral as opposed to somatic activity; or activity of the thalamus as opposed to activity of the cerebral cortex.

2. Degree of arousal, or intensity of reaction, of the organism.

3. Disorganization, and consequent ineffectiveness, of behavior.

4. Interpretative data of various sorts, *e.g.*, descriptions of the content of consciousness, or of the kind of stimulus-response situation.

5. Various combinations of the above differentiae.

I examined each of these types of definition and pointed out its inadequacies—*including that of emotion as disorganization of behavior*. I concluded that "'Emotion' does not represent a unique state; it represents merely one end of a continuum. Or rather, it represents various ill-defined points on a number of continua, according to the definition employed" (3, p. 187). In the summary of the article, I stated, "An examination of these various types of definition of emotion results in the conclusion that in every case the distinction between 'emotion' and other patterns of reaction is one of degree rather than of kind. It further appears that, since the precise degree of a given kind of behavior which is to be called 'emotional' is never stated, the concept is not useful in exact psychological investigation" (3, p. 197).

Since the article has apparently been *widely unread*, perhaps it would be useful to summarize briefly the arguments presented against each of the proposed types of definition. They were, in part, as follows:

1. The first type of definition is inadequate because visceral changes of some sort occur all the time. "The

vegetative mechanism passes through various cycles of increased and diminished activity as it sustains the organism and provides the basis for the energy used in motor response" (3, p. 187). Shall we say that we have an emotion whenever there is an increase or a decrease in energy? But, if so, how *much* of an increase or a decrease?

A variant of this first type of definition, description in terms of the part of the nervous system controlling the behavior, is inadequate because, "unless we are agreed upon a statement *of which responses* are emotional responses, it is meaningless to ask whether these responses are always controlled by a certain section of the nervous system. . . . Neurological description is not psychological description" (3, p. 188).

2. The second type of definition of emotion, that in terms of the *intensity* of the physiological arousal, is inadequate because energy is mobilized in a continuum, ranging from a very low point in sound sleep to a very high point during frantic effort. Not only so-called 'emotional' situations, but also the demands of strenuous physical or mental exertion, result in a high degree of mobilization of energy.

3. The third type of definition of emotion, that in terms of disorganized response, is inadequate because "disorganized responses are clearly found in many situations which would not ordinarily be termed 'emotional,' and well-organized responses are, in the opinion of many psychologists, frequently found in the presence of 'emotional' stimulation" (3, p. 189). Moreover, "organized behavior shades into disorganized behavior, with no gap between the two" (3, p. 189).

4. The fourth type of definition of emotion, that which is based upon an interpretation of either certain characteristics of consciousness or the mean-

ing of a given stimulus-response situation, is inadequate because it can never be made exact, and, basically, it is founded upon conventional usage rather than psychological analysis.

5. The fifth type of definition, which represents a combination of two or more of the types described above, shares the inadequacies of the various definitions which it seeks to combine.

This attack upon the concept 'emotion,' which incidentally had been preceded a very short time before by a somewhat similar attack by Max F. Meyer (8), unseen by me until my own paper was almost ready for publication, had little effect upon textbook discussions of emotion. Hence, in 1941, I attempted in another article to offer an explanation of the experiences commonly called 'emotional' without invoking the concept of a unique condition (4). The stated purpose of this paper was to show that "these experiences, which appear to be unique, are in fact merely manifestations in extreme degree of phenomena which are of very general occurrence, and which follow the same principles of action throughout the continua of their occurrence, rather than different principles of action during the condition called 'emotion' " (4, p. 284).

Also, in 1941, I attempted, in an article in this journal entitled "The Conceptual Categories of Psychology: A Suggestion for Revision," to show that the substitution of a smaller number of more basic categories would eliminate the need for such overlapping categories as 'motive,' 'emotion,' and certain others (5). I proposed, as one of these basic categories, that of *energy mobilization*, or the energy level or intensity of a response, pointing out that changes in energy level occur with changes in the amount of effort demanded by the situation *as interpreted by the individual*. Dr. Leeper will find me entirely in agreement with him on the kinship of

'motivation' and 'emotion.' My statement reads in part: "Changes in energy level are assumed to occur, not only as a part of 'emotion,' but also as a part of motivation. A motive, like any other activity, originates in a certain stimulus situation which represents a disequilibrium of the organism. The phenomena of motivation are said to be (1) the maintenance of direction in behavior and (2) an increase in energy level. These phenomena are similar to those described under the category emotion, though emotional behavior is said to be characterized at times by a *decrease* rather than an *increase* in energy level, and emotion is said to involve a distinctive (undefined) feeling tone. Certainly motives also, as well as all other states of the organism, involve some feeling, or awareness, of the condition of the organism, and the only difference between the feeling tone of emotion and that of other conditions is that the energy level called emotional represents an extreme departure from the usual energy level and involves, therefore, an unusual heightening or deadening of sensations" (5, p. 194).

I hope I have made it entirely clear that, from my point of view, Dr. Leeper's identification of emotion with motivation represents a decided advance over the usual way of regarding the phenomena of emotion. I would cheer him heartily in his attack on the inconsistencies, the ambiguities, and the downright obfuscation of current textbook presentations of the topic of emotion.

But, if I may again take up the fight, I should like to join battle at a point at which Dr. Leeper has laid down his weapons. Not *all* leading textbooks of psychology define emotion as disorganized response. For example, a textbook which is still considered by many psychologists to be the leading textbook in the field, that of Dashiell, defines 'emotion' as follows: "What is the essential nature of those phases of a person's life we call 'emotional'? It may now be formulated. *The changes of diffuse internal conditions in his organism (through external or internal stimuli) act indirectly (neurally) or directly (chemically) upon his organs of overt response. As a result his behavior towards things and people is strengthened and accelerated or is weakened and retarded. He is typically aware of these internal conditions, and may report them verbally as feelings of 'pleasantness,' 'happiness,' 'anger,' 'reproach,' and so on*" (1, p. 163). For Dashiell the distinguishing characteristic of emotion is "changes of diffuse internal conditions" by which behavior is "strengthened and accelerated or is weakened and retarded"—in other words, changes in energy mobilization. The implication is, of course, that there are certain internal conditions which strengthen or weaken behavior enough to be called emotional, and others, presumably, which are non-emotional. But where, or by what criteria, shall we draw the line between 'emotion' and 'non-emotion'? And why do we assume a dichotomy rather than a continuum? Does Dr. Leeper hail Dr. Dashiell as an advocate of a motivational theory of emotion, or does he repudiate also *this* textbook definition of emotion, albeit not based upon the theory of emotion as disorganized response?

Turning to Dr. Leeper's proposed substitute for current theories of emotion, that of conceiving of emotional processes as motives, I search in vain for a definition of *emotional* processes which would distinguish them from *other types* of motives. I maintain that no such distinction can be made. But let us see what Dr. Leeper has to say on the subject. I find the following: "To begin with, we can use the 'pointing' method of defining emotion. We can say that emotions are such phe-

nomena as fear, anger, feelings of guilt, feelings of grief, affection, pride in the doing of good work, enjoyment of beautiful music, and enjoyment of companionship. Continuing our pointing, we may say that 'emotions' are to be seen, not merely in cases where these processes occur in intense form, but that emotion can exist as Woodworth says, in all degrees of intensity" (7, pp. 16–17). Degree of intensity, then, cannot be used to distinguish an emotion from a motive or, following Leeper, from *other* motives. Dr. Leeper continues: "We ask, then, what properties mark all of such a diverse collection of phenomena, at least when they are not carried to rare extremes. We have seen that disorganization is not characteristic. . . . If this line of argument is sound, it means that emotional processes operate primarily as motives. It means that they are processes which arouse, sustain, and direct activity!" (7, p. 17).

Is there any difference, for Leeper, between *emotional* processes and other motivational processes? A difference is implied (else why keep two terms for the same thing?), but no difference is explicitly described. The only further discussion of the topic which I can find reads as follows: ". . . it is quite a possible hypothesis that the perception of the emotion-provoking situation produces the emotional process (which may have a conscious aspect to it, and which may produce also an autonomic discharge, either directly or via some subcortical centers, and which may then be reinforced or supported by widespread bodily changes). And it is quite a possible hypothesis that this emotional process (perhaps as reinforced by interoceptive impulses from visceral reactions or proprioceptive impulses from general tonus changes) then operates to motivate behavior" (7, p. 18). The 'emotional process' is constantly referred to but nowhere defined—unless the previous 'pointing' method of definition be considered adequate. And if it *is* so considered, this means merely that we are in an emotional condition whenever we are in a condition *conventionally classified* as emotional. "It would be the view that emotional processes are one of the fundamental means of motivation in the higher animals—a kind of motivation which rests on relatively complex neural activities rather than primarily on definite chemical states or definite receptor stimulation, as in the case of bodily drives or physiological motives such as hunger, thirst, toothache, and craving for salt. In lower animals, such as a clam, there probably are no such emotional processes but merely the physiological motives" (7, p. 19). Is an emotional process, then, a 'nonphysiological motive'? It is said to rest on "relatively complex neural activities rather than primarily on chemical states or definite receptor stimulation." But what kind of *behavior* or *process is* this process which 'rests upon' these more 'complex neural activities'? Neurological description is not psychological description. If we cannot describe the process, how can we determine by *what* neural or other means it is controlled?

Further on, Dr. Leeper appears to confuse this 'non-physiological' motivation called 'emotion' with the animal's ability to respond to relationships. He says, "As such complex creatures developed, those that were motivated merely by the long-established physiological motives were not so well equipped for survival. The animal that did not make avoiding responses until it was grabbed by an enemy was less likely to survive than an animal capable of reactions of fear that would be set off by relatively slight stimuli" (7, p. 19). It would appear that the defect of such an animal is not one of drive or energy mobilization, but one of inadequate maintenance of *di-*

rection in behavior, due to the inability to *respond to relationships* among 'relatively slight stimuli.'

As I have attempted to demonstrate more fully elsewhere (5, 6), it appears that *all* behavior may be described in terms of the two dimensions: *goal-direction* (including responses to relationships) and *intensity* or *energy mobilization*. No other 'cross-sectional' concepts appear to be needed, though we still require such 'longitudinal' concepts as 'learning' in order to describe *sequential* changes in behavior. So-called 'emotional' responses, like all other responses, manifest both *direction* and *intensity*. Much of the confusion in the current discussions of emotion stems from a lack of recognition of this fact. If we should study behavior from the point of view of its *direction* and its *intensity*, we should be able to solve many of the puzzles which have so long resisted attack by means of the atomistic, overlapping, vague and utterly unserviceable categories which constitute our present 'cross-sectional' concepts in psychology.[1]

[1] There has been no attempt in this paper to offer an alternative theory of emotion. The phenomena of 'emotion' appear to be adequately described by the concepts of 'direction' and 'energy mobilization,' presented

Dr. Leeper is definitely 'going my way,' but I wish that I might persuade him to continue farther down the path with me.

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elsewhere (5, 6). The experiences commonly referred to as 'emotional' I have elsewhere (4) attempted to explain in terms which do not invoke the concept of a unique condition.

"A MOTIVATIONAL THEORY OF EMOTIONS . . ." [1]

BY WILSE B. WEBB

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"That whale among fishes—the theory of emotions" has become no less diminutive as a problem with the passage of fifteen years since Meyer (13) so aptly characterized its nature. Leeper (11) is among the most recent to attempt to encapsulate this hulking concept. In attacking this problem (and incidentally nearly every respectable general psychology text), Leeper places the concept in a motivational framework. The present paper, in general agreement, attempts to extend the criticisms and the theoretical ramifications which were indicated by Leeper.

EMOTIONS AS DISRUPTIVE PHENOMENA

Leeper's primary objections seem to stem from a dissatisfaction with viewing emotions as disruptive phenomena. To quote this author, ". . . emotion produces *organization* rather than disorganization. . . . They [emotions] disrupt preceding and incongruous activities, but all integrating activities do the same" (11, p. 21). Appeal to extreme instances (intense emotions) to maintain the disruptive position or to characterize emotions as accompanying (parasitic) phenomena is logically rejected. Leeper suggests that such a framework of emotions (disruption) stems from a 'bygone rationalistic approach' and is encouraged by our technological culture.

In considering this attack, it should be noted that this battle standard is not a new one nor has it particularly impressed the opposition. Young himself, one of the focal points of Leeper's objections, recognizing the difficulties in forcing emotions into a disruptive paradigm, seems to chafe under his own conclusions. This seems exemplified by the following statements. "The question of whether emotions are disruptive or adaptive is, after all, one of *interpretation* rather than *fact*" (23, p. 448), and "the writer holds to the multiple-aspect hypothesis which postulates that emotional processes simply exist in nature . . . regardless of the ways in which men of science describe and interpret them" (23, p. 460). It is true that Young stresses the communality of disruption as characteristic of emotions; however, he persistently stresses that this behavior is a part of the motivational aspect of behavior. "When emotion occurs there is an imbalance of motives . . . the pattern of emotional expression depends upon the kind of motivation which is thwarted and the nature of the inducing stimulus" (23, p. 478).

More specifically, we find Leeper's attack is historically antedated. Two of the early pioneers in the field of emotions, Darwin (2) and Cannon (1), propounded theories which were the antithesis of disruption hypotheses. Darwin indicated that emotions should be considered as having been derived from biologically adaptive behavior. Cannon's 'emergency theory' of emotions stated that the bodily components of emotional behavior gave evidence of preparing the organism to more readily cope with the emergency leading to such behavior. All research and think-

ing grounded in these approaches has logically been in contrast to a 'disruptive hypothesis.' Most importantly it should here be noted that a critique of disruption has led to no clarity of the concept.

Finally, if attack we must, the level of Leeper's attack does not seem the most appropriate possible. It may be desirable to attack *also* the problem at the general psychology textbook level since future psychologists are there first 'contaminated' with concepts. It is felt, however, that to confine one's attack to this level is to hack at symptomatology. Should not the attack be extended to such fountainheads of the disruptive approach as Watson (22), Laird (10), Higginson (7), and others? Here are the systematic, theoretical, and most critically, empirical aspects that must be encompassed· by any new theory. Only by formulating a concept which will account for their 'disruptive' data, not just damning these data as extremes or fantasy, will we gain agreement. Any other approach can only lead to a choosing of sides or a compromise of ways.

OTHER ATTACKS ON THE PHENOMENA OF EMOTION

In an overview of Leeper's article, it seems that the problem of emotions has been bifurcated along the lines suggested by Rapaport (18), *i.e.*, in terms of the emotions as phenomena in contrast to emotions as a dynamic problem. Leeper has obviously chosen to side with the dynamic approach, and in doing so, has stood on the neck of a single phase of the phenomenological approach, emotions as disruptive phenomena. Before considering Leeper's suggestions concerning a dynamic theoretical approach, other criticisms that have been leveled at the area of emotions deserve at least passing mention.

These further critiques of emotions as phenomena seem classifiable under three general categories:

(1) *Inability to develop an adequate criterion:* Much of the research in emotions has been at this level, attempting to find an index of emotion. Many suggestions have been made and countermanded, *e.g.*, body changes, responses, feelings. None has been established as *the* index of *all* emotions. This inability to 'operationally' define emotions has long been a focus of attack.

(2) *Inability to differentiate between emotional and non-emotional:* Partially as a function of the above listed problem, the problem of defining what is meant by 'emotions' in contrast to 'non-emotional' behavior has long been a fulcrum of criticism. Skinner (19) cites the use of weeping as a definition of an emotional response and further indicates its equivocation by noting that this response may readily occur in the non-emotional situation of having a cinder in the eye.

(3) *Inability to differentiate within emotions:* Because of no method differentiating the wide range of emotions, *e.g.*, joy, fear, rage, etc., the entire concept of emotions as an entity has been questioned. Dashiell (3), and earlier Cannon (1), wrote papers which seem particularly pertinent on this problem.

Examples of the specific instances of these critical evaluations of emotions are too numerous to attempt to consider here. Innumerable experiments have been performed to promote one or more of these critiques. Many laboratory hours have been spent proving that facial expressions can or cannot be differentiated; that bodily reactions are the same or different for different emotions; that the PGR changes are a concomitant or not a concomitant of emotions. Summaries of these 'pro' and 'con' experiments may be found in

nearly any general discussions of emotions.

The ultimate extension of the attack on the phenomenological stronghold may be seen in Duffy's (5) paper (which in itself is a renovation of the earlier Meyer paper). In this paper emotion is described as a relative concept and it is suggested that we consider emotions as a part of a continuum. Specifically, Duffy reviews the so-called differentia of emotions and finds them all an expression of 'degree,' not 'kind,' and further, "since the precise degree . . . is never stated, the concept is not useful. . . ."

An outstanding attempt to patch the phenomenological approach at a descriptive level may be found in the recent article of Hebb (6). Hebb attempts to trace the 'intuitive' base on which he and others have been able to recognize emotions and concludes that their recognition is an inferred concept, the inference being made on the basis of a behavior deviation from the 'normal' (?) level and in terms of the stimulus, the past history or acquaintance with the organism and the stimulus, the response and associated behavior. As we shall see, this 'intuitive' approach approximates the approach suggested by this paper.

THEORETICAL APPROACHES

In suggesting a motivational approach to emotion, we again find Leeper antedated historically. "By emotions I understand the modifications of the body by which power and action is increased or diminished, aided or restrained." This is not a quotation from Leeper's article but rather a paraphrase of Spinoza's seventeenth century writing as cited by Rapaport (18, p. 26). More contemporaneously the approach of McDougall (12) was recognizably motivational. Symonds (20) in his recent book is seen to equate 'preparatory act' with 'emotional tension.'

Other theorists have considered emotion as some sort of energy or drive. Outstanding among these was that of McDougall (12) and Prince's (17) expansion of this thesis. Finally the systematic approaches outlined below which form the base of the present paper all seem to directly or indirectly implicate a motivational approach. We must conclude then that our answer to this problem of emotion cannot be found alone in the label of 'motivational.'

Leeper's original thesis lies in that our failure in the area of emotions has been a failure in theorizing, not in facts. Perhaps, however, a more appropriate analysis seems to be that the failure resulted from a lack of integration between fact and theory. A consideration of a number of theories previous to Leeper's clarion call seems to indicate that motivational theories are not lacking. Further, these theories seem to be similar and only differing in details.

Leeper suggests that 'emotional processes' should be viewed as "one of the fundamental means of motivation in the higher animals—a kind of motivation which rests on relatively complex neural activities rather than primarily on definite chemical states or definite receptor stimulation . . ." (11, p. 19). This is agreeable but is hardly a theory. Has not this approach been agreed with and in addition been somewhat systematized?

Let us consider two such theoretical approaches. An earlier theory was that proposed by Tolman (21). Emotion is defined as "a release of relatively generalized sign-Gestalt-expectations" leading to "incipient movements going-off appropriately" and "resulting sets of organic and kinesthetic sensations." Sign-Gestalt-expectations in the Tolman system are defined as 'cognitions' or 'readiness to respond' in a particular manner. These 'cognitions' result in preparatory or consummatory activities

compatible with these sign-Gestalt-expectations. Finally, these movements and expectations result in certain sensations which may be 'introspected' as emotional 'feeling.' Each emotion is to be defined "in terms . . . of the process' functional character as an immanent determinant of behavior—*i.e.*, in terms of its character as a demand to and from such and such specific quiescences and disturbances plus also its character as an accompanying mean-ends-relations . . . as distinctive and unique 'directions' of behavior." Most important to an understanding of the nature of this definition is to understand that these terms of 'demand' or 'mean-ends-relation' are not mere descriptive terms but are inferred concepts based upon the experience of the organism, presently operating stimuli and physiological drive conditions and their relation to certain response characteristics which are exhibited.

Skinner (19) has proposed a similar theoretical approach. Skinner first, as with Leeper, rejects the phenomenological approach. "The definition of emotion as a response involving certain effectors . . . is by no means rigorous, since there are probably no effectors involved in emotions which are also not involved in non-emotional behavior" (19, p. 406). He then describes emotion as ". . . a state of strength comparable in many respects with a drive. If . . . responses are to be called emotional, it is not because of any essential emotional character they possess, but because they are elicited by stimuli which typically induce changes in reflex strength" (19, p. 407). Further, Skinner states, "The problem is similar in many ways to that of drive. . . . In both cases we must describe the covariation of the strengths of a number of reflexes as a function of a particular operation. . . . The important thing is the recognition of a change in the strength

as a primary datum and the determination of the functional relation between the strength and same operation. . . . An emotion is a dynamic process rather than a static relation of stimulus and responses" (19, pp. 408–409). In summary, emotion is "some more or less temporary state of reduced strength (an increase in strength would fit into the same formulation) that has been related to a disturbing stimulus or some other emotional operation, such as withholding of reinforcement" (19, p. 409).

A summary paper by Hunt (9) expounds a somewhat similar view by examining and integrating the relevant data up to 1941.

A 'MOTIVATIONAL' THEORY OF EMOTIONS

The theories of Tolman and Skinner and the present paper agree with Leeper in considering emotion as a motivational concept. In addition, the theories of Tolman and Skinner seem to furnish a framework for the dynamic definition of emotions. How have these theories defined emotions and how does the present paper hope to define emotions?

It is proposed that emotion be defined as an inferred concept which results in a change in the organism's behavior, the inference of this concept being posited from an integrated examination of the stimulus aspects and the response aspects of behavior.

More specifically, emotion or emotions would be inferred in a situation in which responses occurred that are not directly definable in terms of the existent conceptual properties of habit or drive. Emotions would be defined when the responses were lawfully related to some measurable property of the stimulus (either antecedent to or existent with the response situation).

It must be admitted that this is no 'differential' definition of emotions; the author, if pressed, would contend that

no differential definition does presently exist. It is further admitted that the 'definition' offered is no more (no less) than a description of the now familiar approach utilizing the 'intervening variable' which has profitably been exploited by Tolman, Hull, and Skinner. Such an approach admits no other definition than an experimental one. It is suggested, however, that an experimental definition generated by such an approach may lead to an unequivocal and utilitarian definition of emotion.

CHARACTERISTICS OF A 'MOTIVATIONAL' APPROACH

What would be the nature of the approach generated by the use of such an experimental framework? First, such an approach would demand that experimentation show that we cannot predict the response in a particular situation on the basis of the presently existing concepts, i.e., that emotions are not mere extensions of the presently existing relationships which have been defined among stimulus aspects and motivational aspects and resultant responses. This is to demand of emotion that to be retained as a heuristic concept it justify its existence as a unique variable rather than an instance of some other concept. The drive concept is thus justified from the fact that holding all factors constant and modifying 'drive' operationally (depriving the organism of some goal object) results in variations in behavior.

Secondly, assuming such a demonstration, the 'inferred concept' approach demands that this inferred concept be adequately anchored on both ends (the stimulus and the response ends) and that the relationships be lawfully defined. How would this be done? Does this approach lend itself to the experimental definition demanded?

Fortunately we have examples of the use of such an approach. The feasi-bility and productive character seem to have been demonstrated in at least three instances. Although 'anxiety' and 'conflict' and 'aggression' are not frequently classified under the typical listing of emotional, it is suggested that general agreement could be reached on the proposition that the reactions exhibited may well fit the present phenomenological paradigm of emotions. Three clear-cut approaches to these specific problems of 'anxiety,' 'conflict,' and 'aggression' may be found in the writings of Mowrer (16), Miller (14), and the Yale group, including Dollard, Doob, Miller, Mowrer, Sears, et al. (4). In these instances emotional responses have not been the focal point, but rather an attempt has been made to correlate stimulus conditions (past and present) with the responses of the organism. Although emotional responses of 'fear,' 'anger,' 'rage,' etc., are predominant among the responses occurring, the experimenters have been content to state that certain responses occur as a result of certain antecedent or existent conditions.

More recently Miller has set about to formulate an experimental definition of the acquired nature of fear and in turn its motivational aspects (15). In these studies we find maximal use of an inferred concept to explain behavior and an exemplification of the program proposed in this paper. These applications of the 'inferred concept' approach, i.e., 'anxiety,' 'conflict,' and 'frustration,' seem characterized by the following design. By observation and/or definition a relevant set of 'stimulus' and/or 'response' variables is selected, e.g., 'aggression,' 'anxiety responses,' 'fear,' 'frustrations,' 'conflicts.' These variables are operationally defined and capable of objective measurement. The experimenter or theorist then states a hypothesis or series of hypotheses concerning the relationship existing be-

tween the 'stimulus' and 'response' variables. His task then is to test these hypothetical relationships in designs which meet the requirements of his definitions. The result of these applied approaches has been the attempt to predict behavior (classically emotional in nature) on the basis of certain antecedent conditions (the past experience with noxious stimulus, the competition of response systems or drives, the blocking of instigated activity).

What do these results of the application of the suggested methodology imply for a 'definition' of the concept of 'emotions'? In general, it is suggested that the definition of emotions should be primarily focused on the integration of the antecedent conditions leading to emotional responses. It must be emphasized that the present paper does not necessarily imply that the term of emotion, or the classical names of emotions, be abandoned. 'Rage' and 'fear' admissibly are responses in three paradigms of 'anxiety,' 'conflict,' and 'frustration.' It is important to note, however, that in each case the conditions leading to (and hence defining) these responses are stated in relation to a prediction of these responses. Certainly such a definition of 'rage' and 'fear' seems more fruitful than the present rather sterile discrete examination of responses or physiology alone.

What are the advantages of this projected approach? A primary advantage, of course, is the fact that the definition is founded in an experimental and empirical test of its character and not a rational definition. More general advantages may be cited. It meets the previously offered critiques head-on. It approaches the problem of 'disruption-non-disruption' by saying it must include all types of responses, rejecting neither but necessarily including both. The continuum approach of Duffy and Meyer is acceptable if the data are

shown to be simply an extension of 'non-emotional' behavior. Emotion or emotions as meaningful concepts will result from the empirical demonstration of differences from or similarity with 'non-emotional' behavior. It does not deny but allows full use of what Leeper calls those "little curios of facial or vocal expression, the PGR or EGR," etc., as being well-defined response aspects which should be included (but not deified or reified as emotion *per se*). Most importantly, it is not only phenomenological but *also* dynamic in its approach, *i.e.*, it forces a definition of all the relevant variables (learned, physiological, etc.) and an integration of the variables with the resultant responses or phenomena.

On what basis may we reasonably reject this approach? Basically one may object that emotions could not be related to antecedent conditions. Such objections could only be supported by contending that emotions are unlawful, *i.e.*, independent of antecedent conditions or that the task was too difficult or complex. Accepting either alternative would be to deny basic tenets of the science of psychology and could hardly be defended by a serious minded psychologist. A further and more critical objection may be that such an approach would not 'define' emotion. A 'classical' one-sentence or one-paragraph definition of emotions may well not accrue from the present approach. It is suggested, however, that the prediction and definition of all the responses now classified as emotions would result. If by 'definition' we accept this statement 'complete description,' then it is suggested that the present approach would generate an adequate definition. Finally, it may be maintained that our job is to 'classify' the phenomena of emotions rather than 'predict' emotions. It is doubted that even the most ardent

phenomenologist would maintain such a statement of purpose.

SUMMARY AND CONCLUSIONS

The present paper agrees with Leeper in its rejection of the phenomenological approach and placing emotions in a motivational framework. It is, however, noted that these arguments *per se* have failed to integrate the problem of emotions. Using the previously suggested theories of Tolman and Skinner as basic frameworks, the present paper points to presently existing experimental programs which seem to be particularly applicable to the definition of emotions.

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THE RELATION OF 'SET' TO RETENTION [1]

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Among the conditions which determine the degree of retention of a learned response, the factor of the subject's set to perform the learned activity has received relatively scant attention on both the experimental and theoretical levels. Other conditions of retention such as retroactive inhibition and altered stimulus context have usually received much greater emphasis. McGeoch (7), it is true, has considered inadequate set to perform, along with retroactive inhibition and altered stimulus context, as being one of the conditions of retention, or inversely, of forgetting, but the greater emphasis which he placed upon the factor of retroactive inhibition is more or less representative of the prevailing belief of psychologists concerning the relative importance of the factor of set as one of the conditions of retention.

The emphasis upon retroactive inhibition, to the partial exclusion of other conditions of the forgetting process, has, perhaps, been encouraged by the nature of the typical design of retroactive inhibition experiments. These experiments have usually been conducted in accordance with the general type of experimental plan indicated in the schematic diagram, below.

Rest condition   Learn A Rest   Relearn A
(control)

Work condition   Learn A Learn B Relearn A
(experimental)

It will be observed that this plan provides only a measure of the retention loss in the work condition over and above that which may occur in the rest condition. This loss has been correctly identified as being attributable to the effects of the interpolated learning. This procedure does not provide, however, for determining the amount of forgetting which has occurred under the rest condition. The amount of such forgetting may be estimated by comparing the mean score on the last original learning trial with the mean score on the first post-rest or relearning trial, but the estimate thus obtained is systematically too small since the increment in performance to be expected as a result of the last learning trial is omitted from the computation. Nevertheless consistent amounts of forgetting are noted as between the last original learning trial and the first relearning trial. In Table I are presented a few representative examples taken from the literature. It will be noted, in all cases, that the score on the last trial of original learning is higher than the score on the first relearning trial. [2]

What is needed in order that this decrement may be estimated with accuracy is a modification of the design of retroactive inhibition experiments to include one no-rest condition as indicated in the diagram, below.

No-rest condi-   Learn A No-rest Relearn A
tion (control)

Rest condition   Learn A Rest   Relearn A
(control)

Work condition   Learn A Learn B Relearn A
(experimental)

[2] This decrement occurs whether original learning was to a trial or to a performance criterion, and hence may not be considered to be an artifact of the nature of the original learning situation.

TABLE I

COMPARISON OF PERFORMANCE SCORES ON THE LAST TRIAL OF ORIGINAL LEARNING WITH
SCORES ON THE FIRST TRIAL OF RELEARNING UNDER THE REST CONDITION
IN A NUMBER OF TYPICAL EXPERIMENTS ON RETROACTIVE INHIBITION

Experiment	Type of learning task	Score on last original learning trial	Score on first relearning trial
Gibson (2)	form-word associations	7.91	7.57
Irion (4)	rote-serial learning of adjectives	6.56	5.68
McGeoch and Underwood (8)	learning of paired-associate adjectives	5.54	4.54
Melton and Irwin (9)	rote-serial learning of nonsense-syllables	5.80 7.60	3.50 5.58
Underwood (13)	learning of paired-associate adjectives	6.46 6.33 6.37	4.21 4.38 4.08
Thune and Underwood (12)	learning of paired-associate adjectives	4.96	4.38

Such a design would permit the measurement of the amount of forgetting which occurs as a function of whatever factors operate during the rest period as well as the additional forgetting which occurs as a result of the interpolated learning. Recent evidence (5) would point to the conclusion that a substantial loss in retention may occur during rest periods of the length commonly used in retroactive inhibition experiments. The data in Table II represent the forgetting of a list of fifteen paired-associate adjectives, originally practiced for ten trials. In all cases the rest intervals were filled with cartoon reading. In view of the fact that retroactive inhibition experiments have used comparable lengths of retention intervals, it would appear that a relatively important portion of forgetting may have been neglected under the conventional type of experimental design.

That loss of retention during the rest period does occur has often been noted (although usually underestimated), the usual explanation for this loss being stated in terms of the incidental learning of conflicting associations during the rest period. On the other hand, the re-

TABLE II

RETENTION OF A PARTIALLY LEARNED LIST OF 15 PAIRED-ASSOCIATE ADJECTIVES

Group (all n's = 15)	Mean No. correct anticipations on last original learning trial	Length of rest	Mean No. correct anticipations on first relearning trial	Difference
1	6.80	no-rest	7.53	+0.73
2	6.80	1 min.	7.13	+0.33
3	6.93	5 min.	6.93	0.00
4	6.67	10 min.	6.53	−0.14
5	6.80	20 min.	6.13	−0.67
6	7.00	40 min.	5.80	−1.20

sults of experimentation on incidental learning (6, 10) indicate that conflicting associations which might be acquired in this manner would be poorly learned relative to the habits which had been established during the several trials of formal practice in original learning. It might also be pointed out that the stimuli which would evoke the incidentally learned, and presumably conflicting, responses would, in all probability, be quite dissimilar to the stimuli which would evoke the originally learned responses. The low level of learning of the conflicting associations and the lack of stimulus similarity, noted above, would constitute a state of affairs which should be productive of little interference.

There is, however, a possibility that a large portion of the forgetting loss which occurs under conditions wherein there is no formal practice on an interpolated learning task may be attributable to a loss of set to perform the activity involved. This possibility has been considered by Ammons (1) in connection with his theoretical analysis of the motor learning process. Here use was made of the concept of set in conjunction with the warming-up phenomenon which has been noted in many motor learning situations. According to Ammons' analysis, the warming-up period is identified with that period in relearning, during which the subject regains the postural and attentive adjustments which are necessary for his optimal performance. Conversely, the loss of retention attributable to a need for warming-up is identified with the loss of these set factors during the retention interval.

The phenomenon of warming-up, of course, has long been recognized (11). Ordinarily warming-up has not been taken to apply to retention except as the latter is measured by either the method of relearning or the method of reconstruction. This does not imply that warming-up does not occur within a single recall trial, but rather that at least two measurements are required in order that the warming-up effects may be identified. These effects, as they apply to retention, may be tentatively defined in terms of the greater slope of the initial segment of the relearning curve relative to the slope of the original learning curve at a corresponding level of initial proficiency.

If a portion of the decrement in retention is conceived to be due to a loss of set to perform, this loss of set should itself be a function of a specifiable set of conditions. On the hypothesis that set may be identified with an aggregation of postural and attentive adjustments, it would appear that loss of set should be, primarily, a function of the activity of the subject between the time of original learning and its later recall. If this activity should be of such a nature as to disrupt the set of the subject to perform, retention should be decreased, and the warming-up effect thereby enhanced. On the other hand, should the interpolated activities of the subject be of such a nature as to maintain the set to perform, the amount of retention should be relatively greater, and the warming-up effect should be decreased. Ordinary rest interval activities may serve either to maintain or to disrupt the set of the subject to perform, depending upon the nature of the learning task. Presumably, during the rest period, subjects typically lose the various postural and attentive adjustments which have been established during the period of original learning. It would be expected that rest period activities which are similar to the learning activity would serve better to maintain the subject's set to perform than would rest period activities which were dissimilar to the learning activity. Thus, in the rote-learning situation, for ex-

Table III

Retention under Conditions Where Set to Perform is and is
Not Reinstated Prior to Recall

Group	Mean No. correct anticipations on last original learning trial	Length of rest	Set reinstated	Mean No. correct anticipations on first relearning trial	Difference
A	6.80	no-rest	***	7.53	+0.73
B	6.27	24 hours	no	4.40	−1.87
C	5.80	24 hours	yes	6.07	+0.27

ample, a rest period activity which involved the naming of colors or of figures which were presented on the exposure apparatus or memory drum at the rate of presentation of the materials to be learned would serve to maintain the subject's set to perform more adequately than some more dissimilar activity such as cartoon reading, or even the naming of colors or figures at a different rate of presentation from the learning rate.[3]

It may also be noted that it should be possible to reinstate a subject's set to perform a particular activity by requiring him to engage in some activity, similar to the learning activity, immediately before testing retention. In the case of the rote-learning situation, again, this could be accomplished by having the subject name colors on the memory drum for a short time just before testing retention. This color-naming should serve partially to reinstate the subject's set to perform in this situation in that, under such circumstances, he would probably regain many of the postural and attentive adjustments which were present during original learning. This effect has been experimentally verified

[3] In this connection it may be noted that the design of experiments for retroactive inhibition probably tends to minimize the amount of measurable inhibition because the interpolated learning activity, while introducing conflicting associations, probably also serves, in most cases, better to maintain the subject's set to perform than does the rest period activity.

(5) for the retention of paired-associate adjectives. Three groups of subjects were given the following experimental treatments. One group received twenty trials of practice on a list of fifteen paired-associate adjectives without receiving a rest interval. A second group received ten trials on the same list and, twenty-four hours later, received ten additional trials. The third group also received ten trials on the first day and ten additional trials after a twenty-four hour rest. In this case, however, the subjects were given one trial of color-naming, the colors being presented on the memory drum, immediately before resuming practice on the second day. The data from these groups are presented in Table III. It will be noted that the effect of the single trial of color-naming was to decrease, sharply, the amount of forgetting. When the initial differences between the groups are taken into consideration (as in considering the change in the mean number of correct anticipations from trial 10 to trial 11), this effect is even more strikingly demonstrated.

Following the same reasoning, it should be possible to increase the rate of learning in a new situation by inducing in the subject an adequate set to perform immediately before he begins to learn. Preliminary results from a number of experiments currently being conducted indicate that this increase in the rate of learning a new task may be demonstrated experimentally.

On the basis of such an hypothesis, a number of predictions of experimental results may be made, some of which are listed below.

1. Retention should be greater following the performance during the rest period of a set-maintaining activity than following the performance of a set-disturbing activity. In the latter case, amount of retention loss should be an inverse function of the similarity between the rest period activity and the activity to be retained.

2. Regardless of the activities which occur during the rest period, if, immediately prior to measuring retention, the subject engages in an activity which reinstates an appropriate set to perform, retention should be greater than if this set had not been reinstated. In this case, the extent of recovery from retention loss should be a function of the similarity between the set-inducing task and the learning task, and of the amount of time devoted to the induction of appropriate set.

3. If, prior to original learning, set to perform is induced, rate of learning should be greater than if this set had not been induced. Amount of this increase in the rate of acquisition should be a function of the similarity between the set-inducing task and the learning task, and of the amount of time devoted to the induction of appropriate set.

Loss or disruption of the set to perform could operate to produce a decrement in retention in terms of the operation of at least the following three factors operating simultaneously.

1. Loss of set, as between original learning and recall, could result in the failure of the subject in the recall situation adequately to receive from the environment the necessary stimulus cues for the elicitation of the previously learned response. Even in cases wherein these cues were received, the pattern of proximal stimulation might be quite different as between the time of original learning and the time of recall. This difference in proximal stimulation would, of course, be one result of a shift in the orientation of the subject toward the stimulus objects in his immediate environment. In the latter case, resulting loss of retention could take place as a result of the failure of stimulus generalization to occur.

2. Similarly, the patterns of internal stimulation which become a part of the stimulus complex which acquires, during original learning, an increased probability of response elicitation, might be changed as between original learning and relearning. Such differences, again, could result in a retention loss due to a failure of stimulus generalization to occur.[4]

3. For reasons of the mechanical efficiency of the responding organism, certain postures and muscular tension patterns may be optimal for the performance of a given task. To the extent that these optimal postures and tensions are acquired during original learning and are disrupted between the time of original learning and recall, there may be a decrement in performance which is attributable to this decreased state of responding efficiency.

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HILGARD ON THE DOMINANT LAWS OF LEARNING

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Eighteen hundred and ninety-eight probably marks the first year in what has since become an annual event wherein someone claims the inadequacy of E. L. Thorndike's contributions, and the end, therefore, to his influence. Of the more than half-dozen who are currently attempting to toll the bell for Thorndikian conceptions,[1] it is Hilgard in his recent authoritative book on *Theories of Learning* [2] that will be most widely read and presumably most influential. In evaluating Thorndike's work, Hilgard reaches the surprising and, I believe, inconsistent summation that:

"For Thorndike, the spread of effect is the last line of defense. If it fails there is little of theoretical interest left in what were once the dominant laws of learning" (4, p. 46).

This statement is more damaging than it may at first appear for it applies also to those who have borrowed Thorndike's central conceptions; and the future, therefore, of the contributions of Hull, Skinner, and the current functionalists [3] is dangerously tenuous to say the least. Yet within Hilgard's own book there is ample evidence for the de-

nial of his own generalization and the support of the proposition to which the present writer would hold; namely, that the full impact of Thorndike's work is just beginning to be felt by psychology at large. Consider, for example, this incongruity. Throughout his book Hilgard has noted that the problem most inadequately treated by contemporary theorists is the influence of punishment. Yet the most extensive and significant work on punishment is that of Thorndike himself (14, 16) whose contributions are said to be in danger of having 'little of theoretical interest.'

But there are charges even more serious than inconsistency that can be levelled at Hilgard's generalization. It is based on two fundamental misconceptions of the logic of science.

It is only when one conceives the theoretical interest of the law of effect (or reinforcement) as residing only in its being the single basic qualitative law of selection that it could ever be said to have little of theoretical interest if that defense of the law broke down.[4] But theory is interested in quantitative laws even more than qualitative ones. Even if the law of effect is relegated to a secondary law (where practically no one denies its importance) it can hardly be said to have little of theoretical interest. Note the theoretical interest value, for example, possessed by the facts of periodical effect (or reinforcement), or of punishment, as noted above.

A final point. 'Once dominant laws of learning' do not just die. They are

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[1] Allport (1), Hilgard (4), Jenkins and Sheffield (6), Muenzinger (9), Postman (10), Stephens (12, 13), Tilton (18), Tolman (19) and Zirkle (20). The greatest disagreements are with respect to the influence of punishment. The present writer is not in agreement (14) with the evaluation of the evidence as given by either Postman (10) or Hilgard (4), and is therefore preparing an alternative critical review for publication in the near future (15).

[2] New York: Appleton-Century-Crofts, 1948.

[3] Hull (5), Skinner (11), and Melton (8) have all identified the principle of effect as central to their respective theories of learning. Hilgard notes this.

[4] It is to be noted that Thorndike has recently (17) readmitted the law of exercise as a basic selective law in learning.

replaced or supplanted, else they remain in a dominant position. Hilgard, without explicitly saying so,[5] appears to accept the facts of latent learning as substantiating Gestalt principles *as replacements for effect principles*. The latter could hardly lose theoretical interest unless this were so.

But Gestalt principles and connectionist principles can coexist at different analytic levels of studying events, and need not interfere with each other. The question is one of supplementation rather than mutual incompatibility. I seriously doubt if the latent learning experiment can be considered crucial to substantiate one level of scientific explanation at the expense of another. Nor could the facts of spread of effect (how could the facts fail?) ever make Gestalt principles less effective in theory than they are now. The point is something like the one raised by Skinner (11) in distinguishing the behavioral as compared to the nervous system level of explanatory principles in psychology. Surely Skinner does not conceive of behavioral laws as supplanting laws of nervous physiology or of making them less dominant at their own level. Each level may have its dominant laws. The principles of nuclear physics have little within them to 'explain' the wetness of H_2O, but they do not therefore lose whatever dominance they have at their own level of explanation. It may be the task of science to perfectly correlate and therefore eliminate levels, substituting dimensions instead (2, 7, 8), but no division of science can afford to reject organizing principles because they have failed to account for all events within

the sub-science. The crucial experiment is never crucial if the premises of the two frameworks to be distinguished by it come from different but supplementary levels of explanation (3).

If a choice must be made between higher level conceptions with relatively inadequate quantification (Lewin or Köhler) and lower level conceptions with relatively adequate quantification (Hull or Skinner), some of us would choose the latter. But such a choice need not be made. It will be a sad day in science when the former can *replace* the latter.

But Hilgard may, instead, choose the Tolman (19) conceptions which are not subject to *the charge of inadequate quantification. The choice here is between a point of view which accepts purposive behavior as a datum (Tolman) and a point of view which declares that the purposiveness of behavior itself needs detailed investigation to uncover its situational determiners (Hull). I would cast a vote for empiricism which is always more heuristic than nativism. Let us recall again the cognitive structuring of the dog Hilgard mentions who licks his master's hand immediately after a severe beating.

I may have read more into Hilgard than is there, or than he intended, and it may be that he accepts only one level of explanation of learning events, in which case he would be correct in rejecting connectionist doctrine or Gestalt doctrine on the basis of situational inadequacies. Yet if this is the case he should be opposed most strongly by the gestalters who have argued loud and long against any identification of their level with that of Thorndike's connectionism.

Thorndike's laws of learning have had little trouble surviving in the face of criticism in the past, and all important changes in them have come not from the critics but from Thorndike

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[5] If one subtracts the chapters in Hilgard's book which depend upon the Thorndike laws, only Guthrie (chapter 3) and the Gestalt writers (chapters 7 through 11 or 12) remain. Hilgard suggests that Guthrie's theory is grossly oversimplified and that its author fails to meet the responsibility of working out his basic conceptions (4, p. 74).

himself. I suspect that Hilgard's gloomy prognostication will fare no better than any of the others.[6]

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[6] The future status of Thorndike's general system of learning may be a different matter. Hilgard generalizes the 'flavor' of Thorndike's writings as the direct automatic strengthening of specific connections, with a distorted emphasis on the extrinsic character of satisfiers at the expense of the more important internal relationships between success and what the individual is trying to do, goals which satisfy aroused motives or needs (4, pp. 47, 49). Hilgard cautiously notes that "texts may be cited in proof of the fact that Thorndike knows all this" (p. 49). He is right. The texts begin in 1898 and do not end with the book entitled *The Psychology of Wants, Interests and Attitudes* (1935). Hilgard himself is more reasonable elsewhere (4, p. 347): "Thorndike deserves credit for bringing into the foreground the relationship between learning and motivation . . . a factor which had been neglected. . . ."

Thorndike's principles of belonging, assimilation, spread of effect and readiness all involve extensions of a simple specific connectionism. It is certainly a matter of judgment to estimate the 'flavor' of another man's writings, and Hilgard's judgment, it seems to me, is hardly as important as he would make it. One thing a connectionism does do: It sponsors significant research, not all of which employs counterbalancing techniques, but very little of which, it must be admitted, involves following the subject around with a notebook.

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CHESTER ELIJAH KELLOGG

1888–1948

Every science, every art, has its knowledge-wideners, its knowledge-gatherers, and its knowledge-spreaders. Chester Elijah Kellogg was such a triumvirate-in-one, but primarily found his place among that great body of men whose goal is the dissemination of knowledge.

Kellogg took his A.B. degree in the classics from Bowdoin College in 1911, and went on to Harvard to study psychology under Münsterberg, whom he assisted as a teaching fellow; he received the doctorate in 1914. From that time until his death, except for the period between 1917 and 1921 when he served as a Captain in the United States Army, he gave himself to the teaching of psychology. His first post was at Bryn Mawr College as Lecturer in 1914–15; his second was at the University of Minnesota as instructor from 1915 to 1916.

During the war years he was attached to the War Department Field Service as a Development Specialist in Testing and Grading. Some of his Beta test revisions were incorporated in series and used by the American Army. On leaving the Army he came to Acadia University as Professor of Psychology, in which post he served from 1921 to 1924, and from then on he was a familiar figure on the McGill University campus. Well-read yet exceedingly humble, he was at once an encyclopedia of psychology, ever ready to open the pages of his mind to his packed classes and to the interested student whose imagination and curiosity were thus fired. He was highly critical yet never sarcastic, never overbearing, and was always ready to listen to the imagined triumph of some student's ideas which, to the advantage of the student, were just as often laid to rest in the gentlest manner. As a teacher and aid to the graduate student he was an unfailing comfort both in the frustrations of tedious research work and in the personal travails of everyday life. His goal was to be a teacher, and because of his toil and example, many of his former students have followed in the path he pointed out and are now spread throughout the United States and Canada as distinguished teachers and researchers.

With Canada's entrance into the Second World War in 1939 Kellogg, although always an American citizen, threw himself energetically into the development of the now-famous Army Classification Test, the "Army M." Working in conjunction with Professor George Humphrey of Queens University and Dr. N. W. Norton, one of his former students, Kellogg was instrumental, because of his experience with mental testing in the First World War, in delivering the goods to the Canadian Army in time to meet the increased demands at the beginning of the intensive induction campaign in 1942. In recognition of this accomplishment and of his war effort, and because of his ability as a teacher, in 1945 he was elected the first honorary president of the newly-formed Canadian Psychological Association.

At about this time the increased burden of teaching more classes during the war because of the depletion of staff members called to the service, as well as an unflagging personal war effort, began to break the health, if not the spirit, of Professor Kellogg. Against the advice of his physician he kept on with his heavy duties, although with ever-diminishing strength. With the
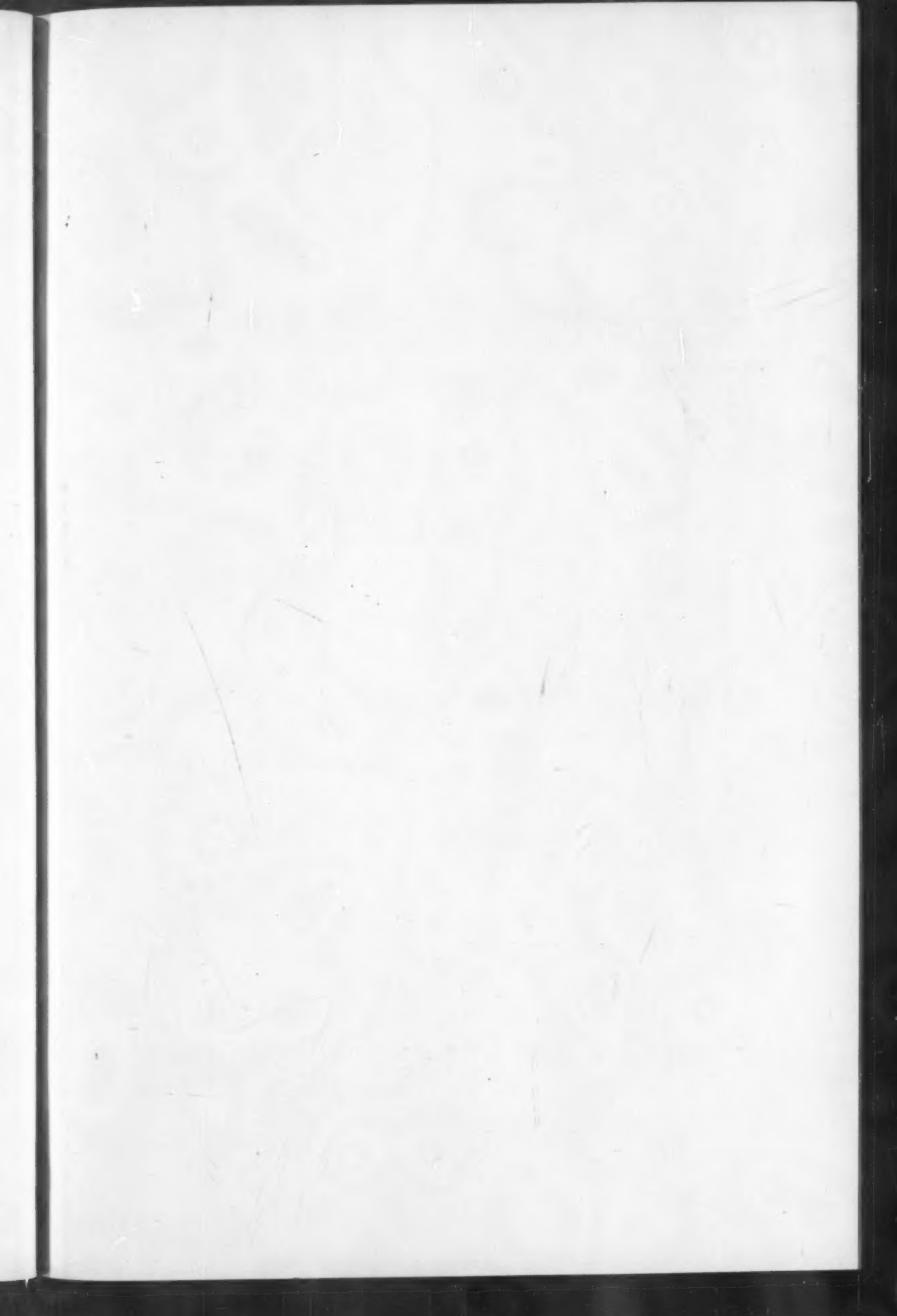
war still not over, he took on the Act-
ing Chairmanship of the Department at
the death of the late William Tate in
1944. This post he held for two years,
at which time his failing health forced
him to withdraw in favor of his former
student, Robert B. MâcLeod. Although
he was very weak from a variety of
chronic complications, for two more
years he tenaciously clung to his life-
long work as a professor of general psy-
chology. In all this time his interest in
mathematical statistics and aesthetics
remained very much alive—to the great
benefit of his students and colleagues.

At the end of the past Spring semester
he was retired. He had hoped to re-
turn with his family to their home in
Maine, formerly the house of his grand-
father, Elijah Kellogg, the writer, but
with the completion of his life work, his
strength, too, was ended; he died on
July 9, 1948.

Professor Kellogg was a good teacher,
but above all a humble man and great
friend, and his memory will always be
thus cherished.

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